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From January 1882 *to March* 1884.

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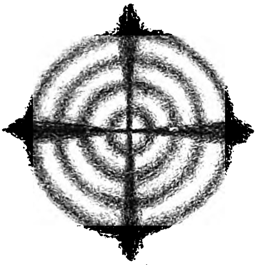

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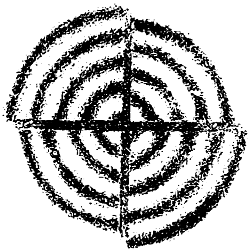

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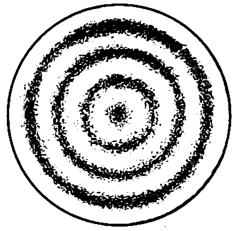

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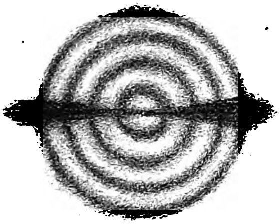

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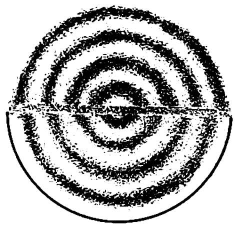

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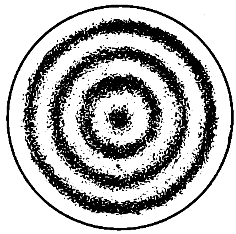

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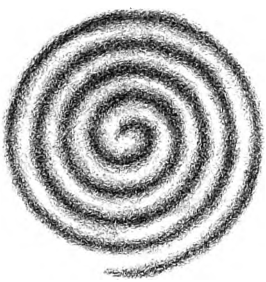

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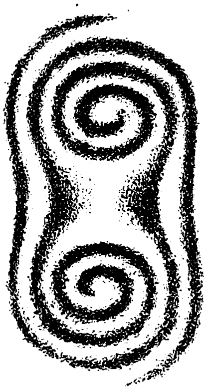

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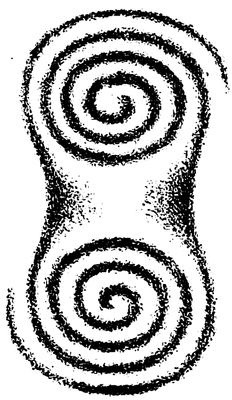


Fig. 12.

Mintern Bros. lith.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

JANUARY 1882.

I. *Some Spiral Figures observable in Crystals, illustrating the Relation of their Optic Axes.* *By* LEWIS WRIGHT*.

[Plate I.]

THE true relation of the optic axes in uniaxial and biaxial crystals has always been an interesting subject. We know that if the crystals be both polarized and analyzed circularly, and we disregard any dispersion of the axes for various colours, the axis of a uniaxial and *one* of the two axes of a biaxial present ultimately similar phenomena. Here, for example, is the well known system of rings and brushes presented by a plate of calcite (Pl. I. fig. 1). As is well known, if we interpose between the polarizer and the crystal a quarter-wave plate, the black cross disappears, to be replaced by grey nebulous lines (fig. 2), on alternate sides of which the quadrants are dislocated; and if now we interpose a second quarter-wave plate between the crystal and the analyzer, when the latter is either crossed or parallel even these lines disappear, and we get simply a series of circular rings with no break whatever (fig. 3). Let us now take a plate of sugar cut across one of its two optic axes. This crystal is suitable for our purpose as having scarcely any axial dispersion, so that one of its axes gives sensible circles, which many other biaxials do not.

* Read November 12, 1881.

Placing it in the stage, we have a system of rings traversed by a straight brush (fig. 4), which, on interposing the first quarter-wave plate, becomes a grey line, on each side of which the semicircles are dislocated (fig. 5); but now interposing the second quarter-wave plate, we have perfect unbroken circles as before (fig. 6).

Now this might seem to imply that the optic axis of the uniaxial calcite resembled in its character that of a *single* axis of the sugar, or biaxial. It need hardly be said here, that such was not the view taken of the matter by those intellectual giants who chiefly shaped into definite form the theory of double refraction in crystals. Gradually this theory was simplified, until Fresnel finally framed the conception of three elasticities within the crystal in the direction of three rectangular axes. If all three elasticities were equal, there was no double refraction; if only two were equal, there was a single axis of no double refraction in the direction of the third; if all were unequal, there were two such optic axes. In any conceivable case the wave-surface could be calculated or geometrically projected upon this hypothesis; and it is needless to repeat how, after its author had passed away, Sir William Hamilton worked out from his conceptions the remarkable and unforeseen results of conical and cylindrical refraction which were experimentally verified by Dr. Lloyd (also removed from us during the past year). That extraordinary verification of Fresnel's theory, which makes the optic axes mere resultants of three rectangular elasticities, has always been considered to have placed it upon an impregnable basis, and seems only to have left for future experiment the possibility of perhaps some further illustration, which is the sole object of this paper.

For observe that, according to this theory, the optic axis of our calcite would *not* correspond in character with a single axis of the sugar or any other biaxial, but must be regarded as simply a limiting case in which *both* such axes coincide. This is well illustrated by the celebrated experiments of Professor Mitscherlich in gradually applying heat to crystals, especially to a crystal of selenite, and thereby altering by the unequal expansion their respective elasticities.

Of the two axes gradually approaching till they unite into one as the elasticities are gradually equalized, there could be

no clearer proof than this old experiment. But it seemed worth while to seek further illustration of one particular point, viz. that the axis of the uniaxial crystal did actually retain or embrace within itself, in some visible form, optical characteristics of the two axes thus brought, temporarily or permanently, into coincidence. This object seemed most likely to be obtained by the aid of quartz, or some other substance possessing similar properties of rotary polarization. Such substances having, apart from their ordinary doubly-refractive effects, two different *axial* velocities or waves capable of being brought into interference, and the two axes of a biaxial being according to hypothesis dissimilar, one being principal and the other secondary to it, it seemed probable that by proper means the two axes might be made to exert some kind of differential or selective action upon the two sets of waves passing through the rotary substance. I was confirmed in this expectation by the curious double spiral, first noticed by Sir George Airy, as displayed by quartz itself when subjected to circularly polarized light, the cause of which appeared to me to be connected with this very matter, as we shall presently see to be the case. After observing with more care than usual, therefore, the effects of quartz in combination with other crystals in various ways, most of which have been described by various observers, I finally adopted the following arrangement:—We introduce first, next to the polarizer, a quarter-wave plate, then (in the convergent rays) a plate of calcite, and next to this a plate of quartz 5 to 7½ millim. thick. The result of this arrangement is the system of double spirals, mutually enwrapping each other, now on the screen (fig. 7). This figure only changes in colour, or moves to or from the centre, as the analyzer is rotated—though there are of course only certain complementary positions of the polarizer, related to that of the quarter-wave plate, which produce them. The point to be here observed is the *double* character of the spiral in this uniaxial crystal.

This figure, however, so closely resembled in all but the number of its convolutions the one exhibited by quartz alone, as described by Sir George Airy in the Cambridge Transactions for 1831, that it might possibly be due to the quartz itself in the convergent light; it was necessary to see if there

were any differential results with other crystals, and, finally, to see if they remained when any such possible cause for them was removed. A single axis of sugar was therefore next placed in the crystal-stage in place of the calcite; and the result is again before you (fig. 8). Observe that, with this single axis of a biaxial, we no longer have the double spiral, but a single one, corresponding exactly to the supposed relation of which we are in search, and also showing that the figures are not proper to the quartz as such, but to some selective action of the axes of the other crystals upon the two axial waves of the quartz and their interferences. A single axis of iron sulphate, and this crystal, cut across a single axis of a topaz, gives similar single spirals.

The single axis being thus tested, we place in the stage a biaxial cut across both axes—in this case nitre (fig. 9). The supposed relation still holds good: each axis now has its own distinct spiral, and the two mutually enwrap one another as in the calcite.

The same is true of crystals whose axes include much wider angles; but to show their spirals we must alter our arrangements. Extra convergent lenses are added in a moment; but if we placed a quartz plate in the strongly convergent light we have to employ to bring both axes of such crystals upon the screen togther, the rings and spirals proper to the quartz itself, which have not appeared in the very moderate convergence so far used, would now appear so strongly as to overpower and distort those due to the crystals under examination. We also want to ascertain beyond doubt that the effects are not due to any *convergence* of the rays traversing the quartz, but solely to selective action upon the right-handed and left-handed waves traversing it axially. We therefore reverse the combination, placing a large plate of quartz about 7½ millim. thick next to the polarizing Nicol, in the parallel rays, and removing the quarter-wave plate to a position between the crystal to be examined and the analyzer. Of course, as it is now the analyzer which is related in position to the quarter-wave plate, the spirals only appear in complementary positions; while, on the other hand, when the analyzer is in those positions the polarizer may be completely rotated. Of course, also, we might have adopted this arrangement all along; but

I have given the experiments as they were made, in order to show how each successive question was determined.

A multiplicity of crystals would be useless: three of various angles will show the uniformity and gradation of the phenomena. Our former crystal of topaz cut across one axis, being thin enough also for the more convergent arrangement, will show that the single spiral appears precisely as before, the convolutions being simply closer in this strongly convergent light. Next we will take again the small angle of another nitre crystal, cut thin enough to show conspicuous figures in the convergent apparatus (fig. 10). Observe that we can now barely distinguish its two spirals, by their oval contour, from those just now presented by the calcite: they are a little *drawn out*, as it were, precisely as we should expect; and that is all; otherwise the visible elements are manifestly the same in both. Arragonite (fig. 11), with an angle of 18½ degrees, shows a spiral of several turns round each axis; but still they finally enwrap each other: and now mica, with an angle in this specimen of 60° or 70° (fig. 12), gives the same phenomena. With the wider separation, the spiral round each axis has room to show separately more of the character of the single axis of the sugar; but the two always preserve the same relation, and only crystals which, owing to very powerful dispersion of their axes, fail to show perfect lemniscates in the ordinary way, fail for the same reason to show these figures complete. Here, for instance, is a plate of borax, whose axial dispersion is considerable and peculiar; but as this still leaves the ordinary lemniscate curves tolerably unbroken, we can trace the spirals without difficulty.

That these figures are solely due to differential action upon the interferences of the quartz rotational colours, we shall demonstrate absolutely in a few minutes; meantime we can almost prove it in two ways. First, though all the arrangements remain complete, the spirals disappear or lose their character in monochromatic light; and, secondly, substituting a quartz of opposite rotation, the direction of the spirals is, as you see, reversed.

And now we will project the beautiful experiment of Prof. Mitscherlich, afterwards applying to it this additional method of analysis. There are the two axes arranged vertically; as

we apply heat they gradually unite. The crystal is now uniaxial. And now the axes open out again, but horizontally. It is a beautiful demonstration, which never loses its fascination for the student. We now add our arrangements for the spirals. There they are, arranged perpendicularly on the screen. They approach as the crystal is heated, till now we have them as in the calcite. Now they open out again in a horizontal direction, like those of a plate of nitre turned round 90° in its own plane. Observe that all through we have the *double* spiral. We can only get a single one by taking a single axis; while the axis of a uniaxial always preserves what we may call its "twin" character. Thus we have the ocular illustration sought at the commencement, of the precise relation predicated by Fresnel's theory between the axes of uniaxial and biaxial crystals, and that the former class do contain, within their single axis, elements (capable of being made optically visible) of both the axes in the latter class.

We have here also objectively demonstrated the reason of the double spiral, first observed by Mr. Airy, in quartz itself. We see that the quartz, considered as an ordinary uniaxial crystal, owing to its peculiar effects upon plane-polarized light passing through it axially, is able to *show its own spirals*, which of course are double. They are not seen at all in parallel light; and, on the other hand, if we increase the rings by convergence, the spirals become more definite. Here, for instance, is a rather thin quartz (one of a pair generally used to show Airy's spirals): in this strongly-convergent circularly polarized light, it shows spirals as well defined as did our calcite. A crucial test of this view readily suggests itself. If it be well founded, obviously we can combine the two properties of our quartz artificially, as it were, since many fluids possess the same power of dividing into two opposite circular waves, differently retarded, a plane-polarized ray. If therefore we take a column of such fluid of sufficient length, and any ordinary uniaxial crystal, the one will represent the peculiar axial properties, and the other the ordinary doubly-refractive properties of the quartz; and the two ought to give similar double spirals. In fact an adequate column of fluid ought to replace the quartz successfully in all the foregoing

experiments. Our last step, therefore, is to prove that this is the case. I have here a tube of oil of lemons 200 millimetres (8 inches) in length, which we introduce into the parallel-plane-polarized beam in place of the quartz, whose axial properties it now represents. In the crystal-stage we adjust the calcite, which in all except being a negative crystal (and a positive one would be just the same) represents the ordinary doubly-refractive properties. And now introducing the quarter-wave plate between crystal and analyzer, we have the spirals as given by the quartz in all respects. The same effects are produced by other crystals, any slight inferiority being due to the slightly yellowish tint of the fluid, which so far approximates to homogeneous light. Spirit of turpentine is free from this defect; but we could hardly project through a tube of sufficient length.

These phenomena hold good through all the ordinary analogies with, or substitutes for, natural crystals. This round disk of chilled glass, placed in parallel light, which behaves in all other respects like a crystal in convergent light, also gives double spirals like those of the calcite. I have here also an artificial uniaxial crystal formed of crossed mica-films, after Norremberg, and an artificial quartz of mica-films, after Reusch, for both of which I am indebted to my kind friend Mr. Fox, who made them with his own hands. The first gives the calcite spirals with a quarter-wave plate and quartz; the Reusch preparation gives the quartz spirals with the quarter-wave plate alone.

These experiments of course add nothing to the theory of the matter, and make no such pretension; their whole interest lies in the visible, ocular demonstration they afford of the truth of conclusions long ago worked out by the brilliant mathematical genius of Fresnel. But to the best of my belief they are new; and if it should prove that some other student has been before me, I hope the beauty of the phenomena may excuse my bringing them before you.

II. *On Integrating and other Apparatus for the Measurement of Mechanical and Electrical Forces.* *By* C. Vernon Boys, *A.R.S.M., Demonstrator of Physics in the Normal School of Science, South Kensington**.

[Plates II., III.]

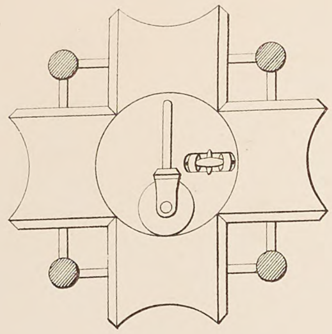
When in February of this year I described my first integrating-machine† before the Physical Society, I felt that, unless the tangent principle could be so applied as to admit of an indefinite growth of the integral, such principle would be useless for practical purposes. In that machine the integral is determined by the *position* of a cart, and so is limited by the size of the apparatus. Since that time I have devised a variety of methods of applying the tangent principle in which the integral is determined by rotation, and so there is no limit to the extent to which the integral may grow. In the following paper, which is divided into two parts, I have given in the first a description of a variety of integrating-machines, while in the second are some useful applications of the most simple form of integrator described in Part I.

Part I.

At the present time there seem to be three types of integrating-machines: 1st, those that may be called radius machines, comprising Sang's planimeter, Clerk Maxwell's sphere machine, and Sir James Thomson's disk sphere and cylinder integrator (Ashton and Storey's steam-power meter also comes under this head); 2nd, sine or cosine machines, comprising Amsler's planimeter and mechanical integrator, and the various wind integrators; 3rd, tangent machines, which, so far as I am aware, are represented only by the cart machine already referred to and those that are the subject of this paper. This class of machines depends on the formula for integration, which, in its geometrical application, finds a curve of which the steepness or the tangent of the inclination (i. e. $\frac{dy}{dx}$) is equal to the ordinate of the given curve or to the given func-

* Read November 26, 1881.

† Proceedings of the Physical Society, vol. iv. p. 199.



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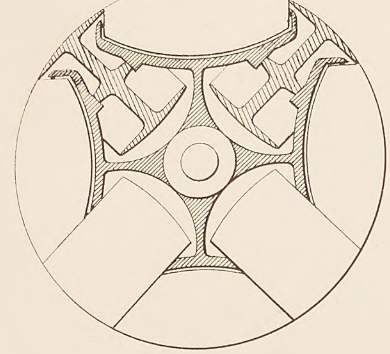

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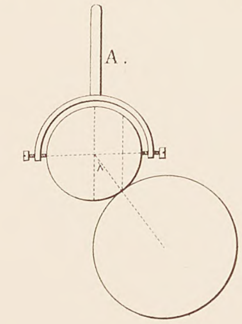

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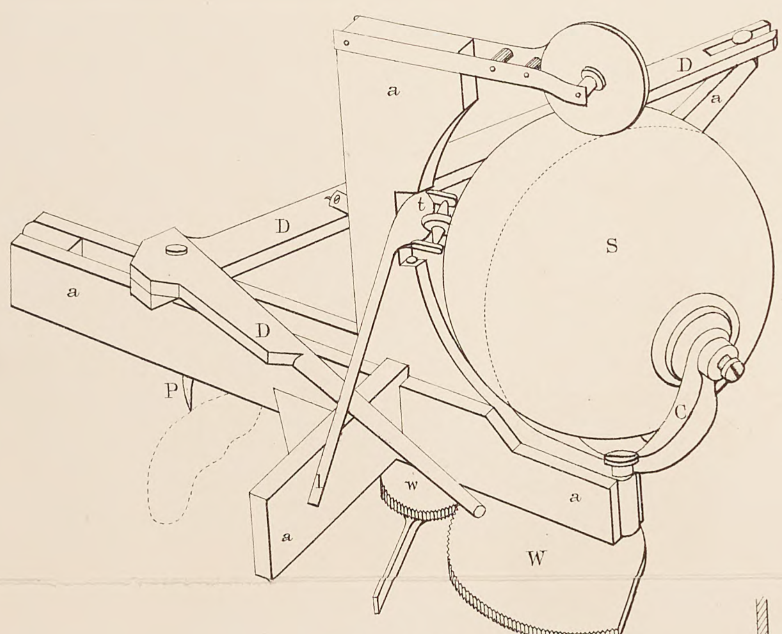

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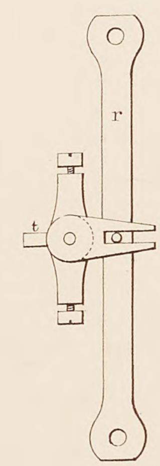

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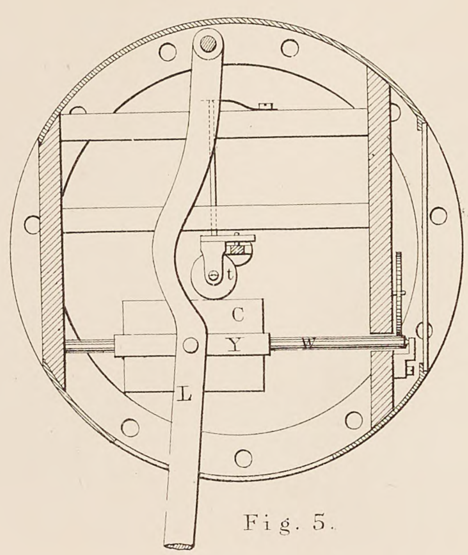

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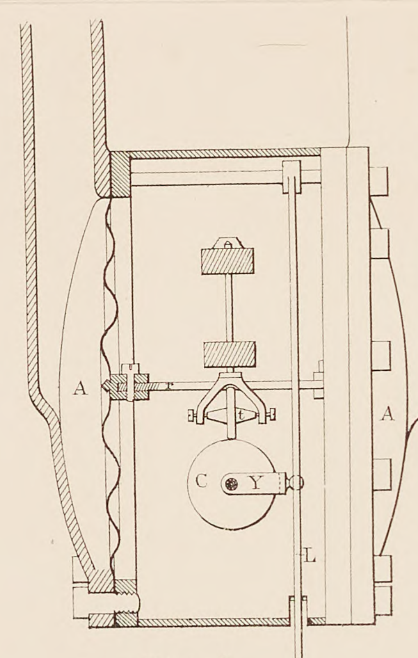

Fig. 6.

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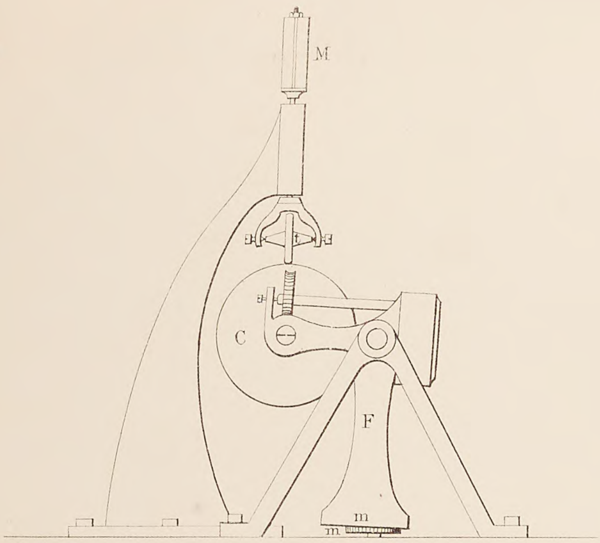

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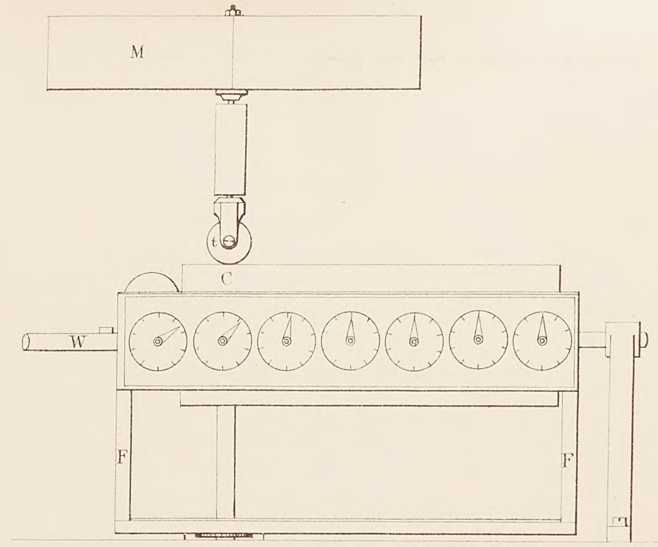

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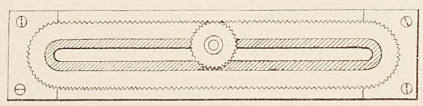

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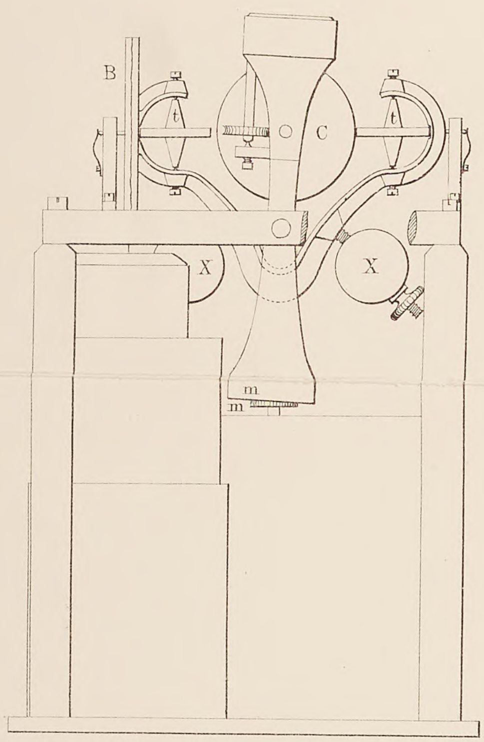

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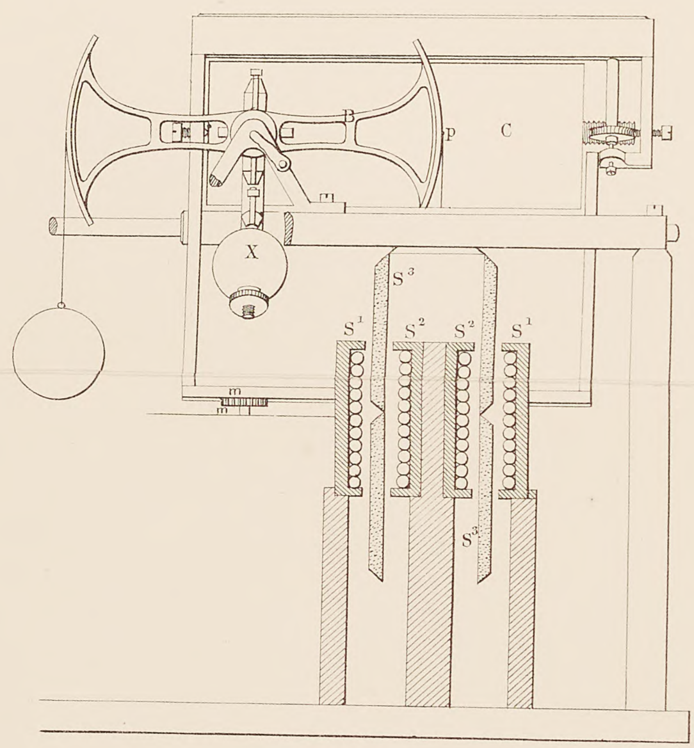

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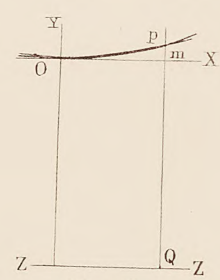

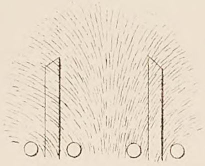

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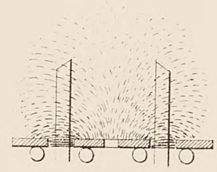


Fig. 14.

Fig. 15.

Mintern Bro's lith.

tion. In my former machine a pointer is made to follow a curve, and by so doing causes a rod to be inclined in such a manner that its tangent is equal to the ordinate. There is also a three-wheeled cart; and the plane of its steering-wheel is by simple mechanism kept parallel to this rod; moreover the horizontal component of the cart's motion is equal to that of the pointer. Under these conditions the vertical component of the motion of the cart (or, shortly, its ascent) determines the integral.

I will now show how this principle is applied in a series of machines in which the integral is determined by rotation and not by linear motion, and in which, therefore, the integral may grow indefinitely. At first let us suppose that the cart in my first machine is incapable of vertical motion; then it, in its attempt to move up or down, will push the paper in the opposite direction. If now the paper is wound on a cylinder with its axis beneath the path described by the front wheel of the cart, and if the hind wheels are supported by some other means, then the cylinder will rotate; and the rate of its rotation will be proportional to the ordinate of the given curve, and the amount of its rotation will be the integral required. Now it will at once appear that the cart and the parallel motion are not wanted, and that the inclination of what was the front wheel of the cart, and what may now be called the tangent-wheel, may be determined mechanically by the same method that was adopted to give inclination to the rod. Also if, instead of moving the tangent-wheel along the surface of the cylinder, the cylinder be moved longitudinally under the tangent-wheel while its inclination is determined by suitable means, then, as before, the rotation of the cylinder is a measure of the integral.

As the cylinder must necessarily have a finite length, it cannot be caused to move continuously in one direction under the tangent-wheel, but must be made to reciprocate. This motion is most readily produced by use of a "mangle-motion," which converts uniform circular into uniform reciprocating motion. Now, when the motion of the cylinder is reversed, so also is the direction of its rotation; and therefore either the action of the tangent-wheel on the cylinder must be reversed, or there must be a reversing-gear between the cylinder

and the mechanism employed to count its revolutions. The action of the tangent-wheel on the cylinder is most easily reversed by having two of them mounted in a frame so that they lie in the same plane, but that one is on one side of the cylinder and the other is on the opposite side. The cylinder is made to bear against one during its forward stroke, and against the other during its return stroke—a change readily effected by the mangle-motion. Then the rotation of the cylinder is independent of the direction of its longitudinal motion. When it is preferred to use only one tangent-wheel, the reversal between the cylinder and the counting mechanism can be easily and perfectly produced by using three bevel wheels and a friction-clutch actuated by the mangle-motion or even by the change of motion of the cylinder.

If the cylinder could be made long enough and the ends bent round so as to join one another, then continuous revolution of the whole ring would take the place of the reciprocation of the cylinder, and the integral would be determined by the rotation of the ring round its circular axis. Such a "smoke-ring" can scarcely be made; but an equivalent can be produced without difficulty. Let there be four barrels, each with a concave instead of a convex outline, mounted on a wheel with their axes in one plane, and with some one generating line on each a quadrant of a common circle concentric with the axis of the wheel. Also let the four barrels be geared to one another by bevel wheels (fig. 1, Plate II.). Now let a tangent-wheel be placed inside the common circle so as to touch it at its lowest point; then, if the tangent-wheel lies in the same plane as the circle, revolution of the wheel supporting the barrels will produce no rotation of those barrels; but if the tangent-wheel is inclined at all, the rotation of the barrels will be directly proportional to the tangent of its inclination, and inversely proportional to the radius of the barrel at the point of contact. This periodical inequality, due to the changing radius of the barrel, may be eliminated by using a second mechanical smoke-ring made of a series of convex barrels mounted on a wheel, with some one generating line of each a portion of a common circle which lies outside instead of inside the barrels. In this, which may be called an outside ring, the other being an inside ring, the barrels must not

be geared together. Fig. 2 is a plan on a larger scale, partly in section, showing how such barrels might be supported. Now, if this outside ring is placed horizontally within the inside ring and touching it at one point, then revolution without rotation of the inside ring will cause rotation without revolution of the outside ring; if, however, in consequence of the inclination of the tangent-wheel, the barrels of the inside ring are caused to rotate, then such rotation will cause revolution of the outside ring; and this revolution will be a true measure of the integral, as the outside ring and the tangent wheel touch barrels of the inside ring at points having the same radius. The astronomical convention with respect to the terms revolution and rotation has been used. By revolution of the ring is meant a turning of the whole round a centre; and by rotation a turning of barrels round their own axes.

Instead of a disk, a sphere similarly mounted may be used for a tangent-wheel, with the same result. Of course the cylinder will be in contact with the sphere at a point on its equator; but if the support of this globe is varied in position, so that the cylinder touches the sphere nearer the poles, then the rate of rotation will depend not only on any former inclination of the plane of the equator of the globe to the axis of the cylinder, but will also be inversely proportional to the cosine of the latitude of the point of contact. The latitude should be brought back to its original value before the rotation of the cylinder is measured. Fig. 3 shows the cylinder in contact with the sphere at a latitude λ. It so happens that the radius in the sphere at the point of contact is equal to $\cos \lambda \times$ the radius of the sphere; but this is not the cause of the introduction of that function, as the rotation of the cylinder is independent of the radius of the tangent-wheel. The true reason can be readily discovered by a simple geometrical construction, which, from the length of this paper, I omit. However, a good illustration may be seen by taking a bicycle and causing it to lean over on its side; then a given twist of the handles will be found to produce a greater deviation in the direction of its motion than would be the case if the bicycle were upright. The effect just described is most easily produced by mounting the cylinder on a rocking-frame, so that it can roll round the ball. Though the axis marked A in the

figure remains vertical, yet the effect may be considered as due to a leaning to one side of this axis. If, however, the axis A is caused to lean forwards or backwards, then the rotation of the cylinder, which is still proportional to the tangent of any rotation about A, is also proportional to the sine of the inclination of A above the horizon; so that if A is horizontal, no rotation about A as an axis will produce any effect; but as A rises higher, increased rotation of the cylinder will be produced, the maximum being when A is vertical. As in the former case, so here, the inclination of A should be brought back to its original value before the rotation of the cylinder is measured. This is the method of steering a wheelbarrow when pushing it. The elevation of the handles corresponds to the inclination of A with the horizon; the equality of the elevation of the two handles corresponds to absence of rotation about A; therefore the barrow goes straight. When turning a corner the outer handle is elevated more than the inner one; this corresponds to rotation about A; and the tangent of this rotation, multiplied by the sine of the elevation of the handles, measures the deviation of the barrow from its straight course. This deviation, then, is greater as the elevation of the handles is greater, and therefore in going round a sharp corner the handles should be raised as much as possible. By the means above described, either the quotient or the product of two functions may be directly integrated.

As in my former integrating-machine, so with these, the reciprocal of a function may be integrated by first inclining the tangent-wheel through a right angle; then, when a function passes through 0 from + to −, the tangent-wheel describes on the surface of the cylinder a cusp showing a momentary infinite motion. A machine so arranged may be used to integrate, not the reciprocal, but the function itself, if, instead of moving the cylinder longitudinally, it is caused to rotate, when the longitudinal motion, or the number of reciprocations if suitable gearing is employed, will measure the integral. In a similar way the machine as first described will integrate a reciprocal.

If for any purpose, in addition to the total result, the integral up to any time is required, a diagram must be drawn. This can be effected by covering the cylinder with a layer

of black tracing-paper, and allowing a band of paper as wide as the cylinder is long to pass between the tangent-wheel and the black surface. The length of paper passed through the machine represents the integral; and the curve drawn shows its rate of growth continuously. Should it only be required to know the amount of growth during each of a series of short intervals of time, a narrow band (which is more manageable) may be used wrapped round a small wheel at the end of the cylinder, and so arranged that at the end of each double stroke of the cylinder it is caused to bear against the point of a stationary pencil; then the pencil-marks represent equal intervals of time, while the distances between them measure the average rate of growth over each interval.

I have at present supposed that the integrating surface is cylindrical; but other surfaces of revolution may be employed for particular purposes. As the rotation of the cylinder depends on the linear motion of its surface, it is clear that its rotation must be inversely proportional to its diameter. If, therefore, instead of a cylinder any other surface of revolution is taken, its rate of rotation will depend not only on the inclination of the tangent-wheel, but also on the radius of contact. The simplest case is that of a disk with the tangent-wheel mounted so as to be capable of radial movement. Then, if the tangent-wheel moves in the direction of its own plane, it will simply describe on the disk a radial line, and there will be no rotation; but if it is inclined at any given angle the disk will rotate, and the rate of its rotation will be proportional to $\frac{1}{r}$, and its whole rotation will be $\int \frac{1}{r}\, dr$, which is $\log r$. Now the tangent-wheel, in its movement outwards, describes on the surface of the disk a spiral which everywhere cuts the radii at the same angle; therefore in such a spiral the angles are the logarithms of the radii; *i. e.* it is the logarithmic spiral. If the inclination of the tangent-wheel is made to depend on some function, then such double-disk machine would integrate $\frac{\phi x}{c+x}\, dx$, in which c is the radius of contact when $x=0$.

If the axis of the tangent-wheel is made to pass through a fixed point over the disk removed from its line of travel by a right angle, then the tangent of its inclination to the direction

of its motion is proportional to the radius of contact: but, other things being equal, the rotation of the disk is inversely as the radius of contact; therefore the amount of rotation of the disk for a given movement of the tangent-wheel is independent of the radius of contact, and the curve traced out on the disk is the spiral of Archimedes. But if, instead of passing over a disk, the tangent-wheel similarly mounted is made to pass along the surface of a cylinder, then the speed of rotation of the cylinder will be proportional to the distance of the disk from its neutral position, and its whole rotation will be $\int cx\,dx$, or $\frac{c}{2}x^2$, and the curve described on the cylinder will be a parabola. This arrangement of the disk and cylinder may be used, as described on page 16, in a polar planimeter to illustrate the formula $\iint r\,dr\,d\theta$.

After the cylinder and disk, the most simple form for an integrating surface is that of a sphere. Let a sphere be supported, with its axis horizontal, on a frame which can be made to reciprocate about a vertical axis which would, if continued, pass through the centre of the sphere; then, if a tangent-wheel is fixed so as to lie, when its inclination is nothing, in the horizontal plane which passes through the axis of the sphere, angular reciprocation, which must be less than 180°, will cause the tangent-wheel to describe on the sphere a meridian when it is in its neutral position, or a rhumb line if inclined at a constant angle. As the speed of rotation of the sphere is inversely proportional to the radius of contact—that is, to the cosine of the latitude of the point of contact—some means must be adopted whereby the rotation recorded is less than the rotation of the sphere in the same ratio. The most simple plan is to use Amsler's principle, and mount a small sliding and rolling wheel so as to be in contact with the sphere at the highest point on the equator (*i. e.* 90° from the tangent-wheel), but with its plane passing through the centre of the tangent-wheel; then the rotation of the Amsler wheel is always less than the rotation of the sphere, in the same ratio that the rotation of the sphere is too great. Instead of an Amsler wheel, a cylinder capable of moving longitudinally on its horizontal axis, and in contact with the sphere at a point exactly opposite to the tangent-wheel, would, by pure rolling

and without any sliding, take off the correct proportion of motion, since it and the tangent-wheel always touch the sphere at points having the same radius.

Fig. 4 is a perspective view of a polar planimeter in which the integration is effected by a disk sphere and Amsler wheel, as described. All the parts marked *a* belong to a rigid frame, which balances on and can turn about a vertical spindle, the top of which is just visible below the tangent-wheel *t*. The vertical spindle is fastened to the stationary wheel *w*, which rests on three feet. The segmental wheel W in gear with *w* is secured to a vertical spindle, the upper end of which carries the crutch C. Screws in the crutch form the horizontal axis about which the sphere S may rotate. The tangent-wheel *t* is mounted in a frame which can be turned about a horizontal axis *e* by means of a lever *l*. The Amsler wheel rests by its weight on the highest point of the equator of the sphere, which is shown dotted. D D is an L-shaped piece, which carries at the angle the pointer P. At the end of the long limb is a slot embracing a pin, as shown. A part of the short limb is made cylindrical; against this part rests the edge of the lever *l*. This edge is not truly radial, but is laterally displaced from the radial position to an extent equal to the radius of the cylindrical part of D D. This causes the true radius, which is parallel to the axis of the tangent-wheel, to intersect the axis of the cylinder. Now, if the pointer P is moved radially in the slot prepared for it, it is clear that the tangent of the inclination of the tangent-wheel *t* will be proportional to the square of the distance of P from the vertical axis about which the machine can turn, also that, during any turning of the machine about this axis, the sphere will turn about its vertical axis at a proportionate speed. Now it has been shown that, when the sphere is made to turn about its vertical axis, the rate of rotation of the Amsler wheel is proportional to such rotation multiplied by the tangent of the inclination of the tangent-wheel—that is, in this case to $r^2\,d\theta$. Therefore the whole rotation of the Amsler wheel is a measure of $\int r^2\,d\theta$; and so, if the pointer P is taken round any closed curve, the area of that curve may be read off from the Amsler wheel. The wheel W is three times the radius of *w*; so that the pointer may, if necessary, be taken completely round the

pole, and yet the tangent-wheel will only move 120° on the sphere in latitude. The diameter of the Amsler wheel is one third of that of the sphere, so as to restore the diminished speed. Unlike Amsler's planimeter, this one shows the increment of area for each part of a closed curve, the reason being that it is an exact mechanical equivalent of the polar formula for integration. Though the machine works very well, it cannot be compared to Amsler's as a practically convenient instrument.

An exact mechanical equivalent of the formula $\iint r\,dr\,d\theta$ would be produced by retaining all the last machine, except the short limb of the L-shaped piece D D, and mounting on the long limb a tangent-wheel to traverse a cylinder, the rate of rotation of which for a given radial movement of the pointer would be proportional to the distance of the pointer from the pole—that is, to $r\,dr$—and the whole rotation would be $\int r\,dr$. Now, if the cylinder were by its rotation caused to change the inclination of the lever l so that the tangent of the inclination of l was proportional to the whole rotation of the cylinder then, when the pointer was taken round a curve, the rotation of the Amsler wheel would be $\iint r\,dr\,d\theta$. In either case, instead of an Amsler wheel, a cylinder mounted as described on the last page would give the integral.

Part II.

The practical value of the tangent principle depends on the fact, that the only operation required of the function to be integrated is that of turning more or less a spindle and tangent-wheel, which may be as light and delicate as any part of a watch, and of which the moment of inertia may be inappreciable. This is in marked contrast to what is necessary in radius machines: the friction in the common double disk or disk and cone integrator, or the inertia of the ball in Sir James Thomson's machine, would be quite sufficient to make the former useless for the integration of such delicate forces as depend on the actions of electricity, or the latter inapplicable to machinery in rapid movement. Another point about tangent machines is, that the whole process of integration is the result of pure rolling, and any doubt that may be felt as to the effect of the sliding action on the accuracy of cosine

machines is here removed. The rest of this paper describes some applications of the disk-cylinder integrator some of which are likely to be of practical value.

Engine-power Meter.

As work is motion multiplied by pressure, the work done in an engine may be found by integrating the difference of pressure on the two sides of the piston with respect to the motion of the piston. For any one stroke, this is usually done by measuring the areas of the indicator-diagrams, one taken at each end of the cylinder, and repeating, so as to get an average value. But as the work done, or the area of the diagram, is subject to variation depending on the load, pressure, and speed, only guesses can be made as to the whole amount of work that has been done by an engine during any length of time. Any machine, therefore, that will automatically find the total work done should be of value, not only to users of engine-power, but especially to experimentalists who are engaged on testing the efficiency of engines, and on other subjects where total work done should be known. It is only fair to mention that Messrs. Ashton and Story have an engine-power meter in which the integration is effected by a double-disk integrator acting on the radius principle; but it necessarily suffers from the defects common to all radius machines. The disk-and-cylinder is especially applicable to this particular case; for it is only necessary to make the cylinder reciprocate with the piston of the engine, the motion being of course reduced to a convenient amount, and to make the tangent of the inclination of the disk vary with the difference in pressure on the two sides of the piston of the engine. Then, at any moment, the cylinder will turn with a speed which is proportional to the rate at which work is being done, and the number of revolutions, as measured by a counter, will be a measure of the work done in foot-pounds or other units during any time. Figs. 5 and 6 are views of an engine-power meter, each partly in section. A, A are two boxes with flexible covers, like the corrugated plate in an aneroid barometer. They may be filled with a mixture of glycerine and water or other liquid, and connected each with one end of the cylinder of the engine. Each diaphragm will feel the pressure, but not the heat, of the

steam or gas in the cylinder. The two diaphragms are connected by the rod *r*; and so the effective force tending to bend the diaphragms is the difference of pressure at the two ends of the cylinder. This is the force acting on the piston of the engine. Inclination is given to the tangent-wheel *t* by the rod *r* by a pin working in a radial slot, as is better shown in fig. 7. This arrangement causes the tangent of the inclination of the tangent-wheel to be proportional to the displacement of the rod *r*, and so to the force acting on the piston. C is the integrating-cylinder, which is capable of sliding along, but of turning with a wire W, which may be grooved or polygonal; pinion-wire is very suitable. The integrating-cylinder is caused to reciprocate, by means of a yoke Y and lever L, in time with the piston of the engine. The stroke is reduced to a convenient amount by attaching a string from the piston-rod to a suitable part of the lever L. Now, as the rate at which the cylinder turns is proportional to the longitudinal motion of the integrating-cylinder multiplied by the tangent of the inclination of the tangent-wheel, and as this is proportional to the motion of the piston multiplied by the force urging it, *i. e.* to the work being done, the whole number of the revolutions of the cylinder will measure the whole amount of work done. If the instrument gets out of adjustment so that the tangent-wheel is not parallel to the axis of the cylinder when there is no force, then whatever error it makes in a forward stroke it will take off in the return stroke; so that no accumulating error will be produced. The diaphragms may either be made of steel or highly elastic metal, in which case they form their own springs; or a softer metal, controlled by an external spring, might be used. If a diagram is required, one may be drawn as described on page 13. But it will not be a diagram such as is drawn by an ordinary indicator, but the integral curve of such a diagram; so that force, instead of being represented by the length of an ordinate, will be represented by steepness. Instead of diaphragms as described, spring pistons or Bourdon pressure-gauge tubes might be employed to give inclination to the tangent-wheel.

Integrating Dynamometers.

The disk-cylinder integrator may be applied to measure the whole amount of work transmitted by shafting or belting. In

the case of shafting, what is called a differential coupling—that is, a contrivance which transmits any motion, but measures the force causing such motion—is employed to give inclination to the tangent-wheel. In the case of belting, any of the known dynamometers may be employed for the same purpose; while a mangle-motion driven by the revolving shaft or travelling band, causes the cylinder to reciprocate. Either of the methods given on page 10 may be employed to produce continuous growth of the integral in one direction. As the work transmitted at any moment is force multiplied by motion, and as the tangent of the inclination of the tangent-wheel is proportional to the force, while the reciprocating motion of the cylinder is proportional to the motion, the rate of revolution of the integrating cylinder will be proportional to the rate at which work is being done, and the whole number of revolutions will give the whole amount of work done. If at any time the force causing the motion should change sign and so resist it, as is the case in an engine when there is much cushioning, then the tangent-wheel will incline the other way and take off from the record a corresponding amount of work.

Electric-Current Meters.

The application of the disk-cylinder integrator to an electric-current meter is very obvious. Figs. 8 and 9 are two views of an electric meter, in which the inclination of the magnet M is effected by the electric current passing in a large coil surrounding the instrument. The magnet M and the tangent-wheel t are each fixed on the same spindle, which is vertical, and which is very light and delicate. The weight of the magnet produces the necessary pressure between the tangent-wheel and the integrating-cylinder C; and as the surface of each is convex, the friction resisting the turning of the tangent-wheel by the magnet is very small. The cylinder is supported in a bell-crank frame F, which can be made to reciprocate along the wire W by means of the mangle-motion $m\,m$. The mangle-motion is actuated by clockwork, which may be wound by the current itself when necessary, should such a course be desirable. Fig. 10 shows the construction of a suitable mangle-motion. The pinion can turn, but not move otherwise, while the frame carrying the racks can move either longi-

tudinally or laterally. A pin projecting centrally from the pinion enters the slot, which is shaded in the figure, and so causes the pinion to gear with the two racks alternately. This lateral movement of the rack-frame is made use of to depress the integrating-cylinder during its back stroke, at which time the magnet rests on the shoulder S. As the tangent of the inclination of the tangent-wheel is proportional to the current-strength, and as the rate of rotation of the cylinder is proportional to the tangent of the inclination of the tangent-wheel, the cylinder will turn with a speed which is proportional to the current-strength, and the whole number of turns, as shown by the counter, will be a measure of the quantity of electricity that has passed. In the arrangement described, time is divided into a great number of equal intervals, and the current-strength during each alternate one considered. After any considerable time, such a sampling of the current would give just as exact a result as would be obtained by integrating continuously. A quick-return mangle-motion might be employed to diminish the proportion of ineffective time; or the whole time could be made effective by keeping the cylinder in continuous contact, and actuating a reversing-gear between the cylinder and a counter by means of the lateral movement of the mangle-motion. It would be well to employ a catch on the armature of a subsidiary electromagnet, so as to stop the clockwork, except when a current is passing. This current-meter, like Edison's electrolytic meter, is a direction-meter. If the current is passing in one direction, it counts it positive; if in the opposite direction, it counts it negative. A reverser actuated by a polarized armature could be employed to make the meter count as positive, a current passing either way, and so make it applicable to the case of alternating currents. Another kind of current-meter, which is by its nature independent of the direction of the current, would be preferable to the magnetic-needle meter and reverser combined.

Figs. 11 and 12 represent an electric-energy meter, which will be described later; but they will serve as diagrams to illustrate a description of the second current-meter. *m m* is the mangle-motion, which causes the cylinder C to reciprocate and bear alternately against the two tangent-wheels *t t*. These wheels are mounted in a common swivelling-frame,

which ordinarily is kept vertical by the weights X X, but which may be inclined by a force due to any cause tending to turn the beam B. Now the turning-power of the weights X X varies as the sine of the inclination; while the turning-power of a force acting downwards applied to the point *p* varies as the cosine of the inclination; therefore the beam will set itself at such an angle that the tangent of the inclination is proportional to the force. If, therefore, the point *p* of the beam can be pulled downwards by a force which is proportional to the strength of the current, an electric meter will be the result. The coils shown in the diagrams, which belong to the energy-meter, must be removed and replaced by an electromagnet and armature of peculiar construction. Let there be an electromagnet with pole-pieces a certain distance apart, and let there be between them a wedge of iron at its narrow end increasing in thickness or width rapidly, and towards its thicker parts much more slowly; then, on moving such a wedge forwards between the poles, but without touching them, it will at first facilitate by its movement magnetic induction at a great rate; and as it fills up the space, even though the induction through it is greater, yet the increase of that induction is less. Now, as the rate at which magnetic induction is increased by movement measures the force with which such a wedge is pulled forwards, the wedge will, if suitably formed, experience a force with a given current-strength which is less as its entrance is greater, except over a small space near its starting position, where the force should, if possible, be infinite. Also, if the wedge is fixed in position and the current made to vary, it will, so long as the magnetic limit is not approached, experience a force which varies as the square of the current; therefore, if the motion of the wedge is resisted externally by a force which varies as its displacement, it will enter to such an extent that the amount of its entrance is proportional to the current. Let such a wedge be carried by the beam B, so that when it is at its zero position the beam is horizontal; then the inclination of the beam will be greater when the current is greater, and, except with very weak currents, may be made to vary so that its tangent is proportional to the current. The same principle might be applied in another way by causing rotation instead of linear movement of

the armature to facilitate induction, and by resisting such motion by a pendulum-weight as before, or by a hair-spring. The armature then would have to be shaped something like an S, and adjusted experimentally so as to give, except with very small currents, a deflection whose tangent is proportional to the current. In Sprague's or Edison's electrolytic meters (the only ones of which I have heard which can lay any claim to being called current-meters at all) a portion only of the current is sent through the meter, and the rest passed by in a shunt. Now, if it were certain that the same proportion of the whole current always passed the meter, there would be no objection to such a course; but as a rise in temperature makes an electrolyte a better, and a metal a worse conductor, any electrolytic meter combined with a shunt would have a tendency to show too much in warm weather or if warmed by the current. If, again, there is any polarization, and that polarization is not strictly proportional to the current, then another error will be introduced. It would seem therefore necessary, if accurate indications are required, to make the whole current pass through an electrolytic meter.

Electric-Energy Meters.

Since the energy expended by an electric current between any two points is equal to the current multiplied by the difference of potential of those points—that is, to the main current multiplied by a shunt current passing in a wire of high resistance between the two points—an electric-energy meter may be made by combining two electric-current meters, which take account of the direction in which the electricity passes, in such a way that the first integrates the main-current strength, and that the mangle-motion of the second is driven by the integrating-cylinder of the first: then, if the needle of the second is deflected by the shunt current, the rotation of the second cylinder will give the integral of the energy expended. This is obvious; for the rate of rotation of the second cylinder is proportional to its rate of reciprocation multiplied by the tangent of the inclination of its tangent-wheel—that is, to the strength of the current in the first machine multiplied by the strength of the current in the second; so its rate of turning is proportional to the rate at which energy is being expended,

and its whole rotation is a measure of the total energy. If at any time either the main or the derived current, but not both, changes sign, then the second cylinder will begin to turn the other way, showing that the current is not doing work in the portion of the conductor between the points, but is being caused to flow by an action of some kind taking place in that portion. If both currents change sign, then, as before, work is being done; and though the first machine is working backwards, the second is working forwards. A combination of two machines, as described, would integrate both the current and the energy. A more simple and practicable machine for integrating energy alone is shown in figs. 11 and 12. The integrating mechanism has been already described; the electrical principles employed must now be explained. If two wires, one conveying the main current and the other the derived current, are near one another, they will attract or repel one another with a force which is proportional to the product of the two currents—that is, to the energy being expended; but if the wires are allowed to move, the force will depend also on the position. The question then is, how can the wires be so arranged that the force exerted may be used to incline the tangent-wheels and yet be independent of their motion. The arrangement of solenoids shown in section in fig. 12 is a complete and perfect answer. S_1 and S_2 are two fixed solenoids concentric with one another; and the main current is made to pass through each in the same direction. S_3 is a solenoid made of a very great length of fine wire, preferably of aluminium silver, as suggested by Mr. Imray; and the upper half is wound in one direction and the lower half in the opposite direction; and the derived current is made to pass through it so as to pass in the upper half in the same direction as the main current passes in the fixed solenoids, and in the lower half in the opposite direction. This solenoid is hung in the annular space between the fixed ones by a band passing over the arc of the beam B. The tubes within and without the fixed solenoids and the rings above and below them, all of which are shown dark in the figure, are made of iron, and may or may not be used according as the currents employed are in general weak or strong. But whether the tubes are or are not retained, the use of the rings has certain advan-

tages, which I now proceed to explain. Fig. 13 is a diagram taken by iron filings, which shows the distribution of the lines of force due to the fixed solenoids alone, without the tubes or rings. Here the lines of force cut the walls of the solenoid at an oblique angle, so that the force, which is at right angles both to the wires and the lines of force, tends to stretch the movable coil, and only a portion of it is effective in dragging down the solenoid: moreover the lines of force are very widely distributed over the solenoid, so that, unless it is of very great length, the upper part will leave by its motion many lines. It is true that on the lower end the movable solenoid will, when at its central position, enter as many lines in an element of motion as it leaves at its upper end; but after any considerable movement the upper end will leave many more than the lower end enters; and the force, as it is proportional to the number of lines enclosed, will become less as the solenoid moves from its central position. This will be referred to again later. Now the iron rings act as traps, so to speak, and catch nearly all the lines of force which without them stray over so great a space. Fig. 14 shows the field produced when the iron is present. It will be seen at once that nearly the whole of the induction takes place across a very narrow band of the solenoid, and that the lines of force, where they cut the solenoid, are nearly horizontal; so that practically the whole of the force developed tends to drag the solenoid downwards, instead of being partly spent, as before, in producing a stretching strain. The consequence is that the movable solenoid may be made very much shorter than would be necessary if no rings were present. The tubes, having a higher coefficient of induction than air, merely serve to increase the number of lines of force, rather than affect their distribution.

If a movable solenoid, arranged as described, is hung by a band passing over the arc of the beam B, then the turning moment due to a down-pulling force is, within certain limits, constant, while the force restraining motion varies as the sine of the inclination of the beam; but had it been hung from the point p, the turning moment would have been proportional to the cosine of the inclination, and the tangent of the inclination would have measured the force. What is wanted

is to make the tangent of the inclination proportional to the product of the two currents; and, as explained, this could be attained by using a long solenoid hung from the point p. But such an arrangement causes a double inconvenience; for not only is a long solenoid itself inconvenient, but the fact that it is hung from an arm and not from an arc causes a lateral shifting of the solenoid when the beam B is inclined, thus necessitating a wider annular space for it to pass through. Now the use of a comparatively short solenoid hanging from an arc introduces two errors which are almost absolutely equal and opposite. The error due to the arc is an increase of moment in the ratio of the cosine of the inclination of the beam to 1; that is, the error is equal to the versed sine of the inclination; and, like a thing that increases as the square of a quantity, it is at first quite inappreciable, and it increases in amount with increasing speed as the quantity grows. Now the error due to a short solenoid, such as shown in the figure, is at first nothing; for a given movement will cause the solenoid to enter as many lines of force at one end as it leaves at the other, but as it gets displaced it enters rather fewer than it leaves; and this difference in the number of lines of force increases in amount with increasing speed. Now, if the proportions are so taken that when the inclination of the beam is a little less than the greatest amount permitted to it the actual magnetic error is equal and opposite to the versed-sine error, then, since each is a quantity which grows according to the same kind of law, those errors will be always approximately equal, and their differences very small compared with the errors, and absolutely inappreciable in comparison with the quantities of which the errors themselves are small. The geometrical representation (fig. 15) makes this clearer. The two sets of errors may be considered as ordinates of two curves drawn to the same abscissa. The point p, where the curves intersect, corresponds to that inclination at which the two errors are made equal, and the origin O to the central position. Since both errors are of a kind which are inappreciable at first, the two curves will have the axis of x as a tangent; and since the two errors are of a kind which grow in the same kind of way, *i. e.* at first slowly, and at an increasing rate as they grow, the curves must be nearly similar;

and since they have the common point p, they must be nearly identical. The distance between the curves at any part gives the actual error there; and this being small in comparison with the ordinates at that part, is inappreciable in comparison with the whole distance of that part from the line $z z$. In the particular case the ordinate $p\,m$ is about 4 per cent. of p Q; so the actual error may be safely neglected. But, finally, should there be any error that can be detected at all, such error may be almost entirely eliminated by putting a few extra turns of wire near the ends or the middle of each half of the movable solenoid, according as the curve of the magnetic error between O and p is below or above the curve of the versed-sine error.

Assuming the truth of what is stated in the last paragraph, we find the tangent of the inclination proportional to the rate at which energy is being expended. But the speed of rotation of the cylinder is proportional to the tangent of the inclination; therefore the number of turns of the cylinder, given by the counting-mechanism in the box over the cylinder, is a measure of the total energy expended. As in the other energy-meter, so in this, if the electric current is helped at any time instead of being used, then the integrating-cylinder will turn the other way, and take off from the record an amount which is a measure of the work expended on the current.

There is a point about the solenoid energy-meter which is worthy of notice. The movable solenoid has an equal number of turns in opposite directions; so it is independent of the magnetic field in which the instrument is placed, and so this meter may be used in workshops or near dynamo-machines without its action being interfered with. For the same reason, when used in houses as gas-meters are for gas, it will be impossible for the householder to tamper with its indications by placing magnets round the instrument.

When very powerful currents are employed, it is well to shunt a certain proportion of the main current past the meter, or, when the electromotive force is very great, to introduce into the secondary circuit resistance-coils. To prevent waste of clockwork energy or of electricity, the main current is made to pass round a subsidiary electromagnet whose armature allows the clockwork to go only when the main current is passing. This armature also, on being attracted, completes

the secondary circuit, so that the derived current can only flow when the main current is passing.

The meter may be used as an energy measurer or indicator (not meter) with advantage when setting the carbons of an electric arc. If the two poles are made to touch, though the current is enormously increased, the energy is reduced, owing to a greater falling-off in the difference of potential between the poles. As the carbons are gradually separated the current diminishes, but the difference of potential increases in a higher ratio; so their product increases; that is, the energy expended, and so the heat and light produced, increases. This increase is shown by an increased inclination of the beam. After a time a point is reached at which the current decreases in the same ratio that the difference of potential increases; at this point the inclination of the beam attains a maximum; beyond this the decrease of the current is in a higher ratio than the increase in the difference of potential, so the energy, heat, and light fall off, as is indicated by the diminution of the inclination of the beam. If, therefore, the carbons are placed so that the inclination of the beam is a maximum, then the best effect is being obtained. In the same way, if the current is being employed to drive a machine, the most effective speed for that machine may be found by observing at what speed the inclination of the beam is greatest.

The various meters described depend for their numerical results on two things—(1) the horizontal intensity of the earth's magnetism, (2) the force of gravity. The indications of the first current-meter are inversely proportional to the horizontal intensity, and of the first energy-meter to the square of the horizontal intensity. Each of them is disturbed by changes in the direction of the earth's magnetism.

The second current-meter and the second energy-meter are independent of the magnetic field altogether. Their indications vary inversely as g when the clockwork is regulated by a balance-wheel, or inversely as $\sqrt{g}$ when a pendulum-clock is used. This dependence on gravity is a point of very great importance; for over any one country gravity does not change appreciably, nor does it matter in what direction the machine is placed so long as it is level. By screwing the weights X X up or down, so as to decrease or increase the influence of

gravity on the meter, its indications may be regulated to a standard measure. Therefore, in making the coils, there is no necessity to count the number of turns exactly, or to lay them with the utmost accuracy: they may be wound in the ordinary way, and then a hundred machines or more connected together, with the main circuits in series and with the derived circuits in series, and a current sent from a suitable source through each series; then, if there is one meter which has been standardized by careful experiment, all the rest can be regulated, just as clocks are, by screwing down the weights X X of those that are going fast, or screwing up the weights of those that are going slow.

If in the foregoing paper any of the apparatus is not as fully described as it might be, I must plead as an excuse an endeavour to occupy a reasonable space with an account of what is essentially one invention.

III. *Apparatus for calculating Efficiency.* By C. VERNON BOYS, *A.R.S.M., Demonstrator of Physics at the Normal School of Science, South Kensington*.*

[Plate IV.]

IN a previous paper I have shown how work done in an engine or transmitted by shafting or belting, or expended by an electric current, or how the quantity of electricity which has passed in a conductor during any time, may be automatically measured and integrated or recorded. The present paper refers to apparatus for dividing rates of growth of two integrals so found one by the other, and continuously recording the quotient. Before describing any of these machines, it may be well to give an example showing an application of a divider to some useful purpose. Let there be a steam-engine driving a dynamo-electric machine, which is employed to produce an electric light. Steam does work on the piston of the engine, which may be integrated as already described. This is the work put in. The electricity does work in the electric arc and in the conducting wires, which may be

* Read January 28, 1882.

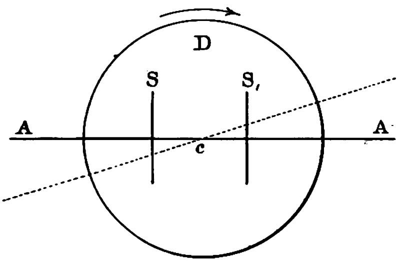


Fig. 1.

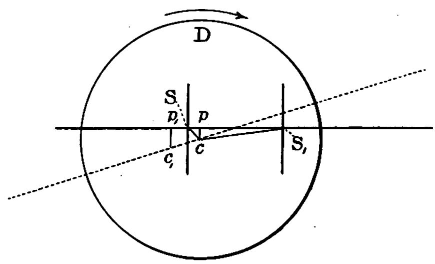


Fig. 2.

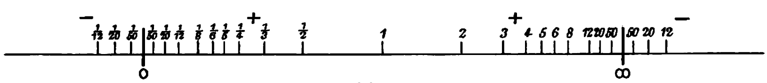


Fig. 3.

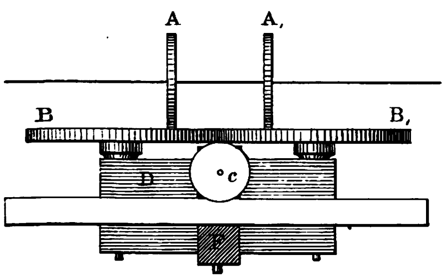


Fig. 4.

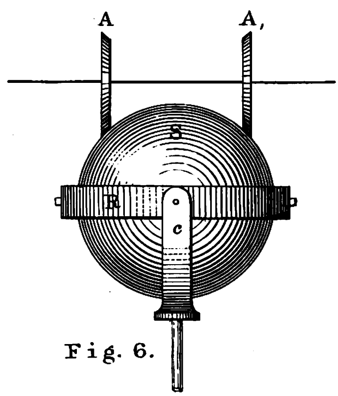


Fig. 6.

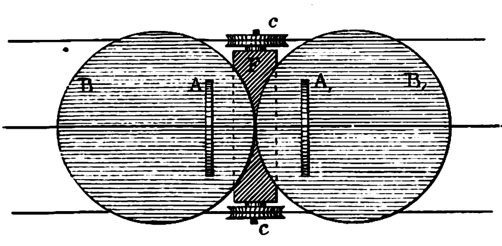


Fig. 5.

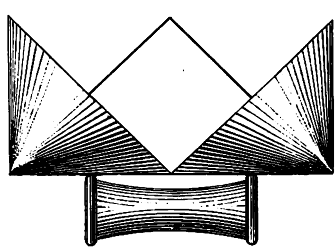


Fig. 7.

Mintern Bro's lith.

integrated. This is the work taken out. If after any time, say one hour, the work taken out is divided by the work put in, the quotient will represent the average efficiency of the engine and machine combined during the hour. In like manner, if the readings are taken after a minute, the quotient will give the average efficiency during the minute. If instead of a minute an indefinitely short period of time is occupied, then the quotient obtained will give the true efficiency *at* that time. Now, if by mechanism or otherwise a curve can be drawn in which the ordinates represent the true efficiency, while the abscissæ are time, then an inspection of the curve will show exactly how well the machines have done their work at every moment, and the highest points will indicate the time at which the best results have been obtained. What is wanted in practice is not a curve giving the true efficiency as above described, because work is not put into an engine uniformly, but intermittently, but a curve showing the average efficiency for the last few seconds or minutes as the case may be; and it is this that the mechanism I am going to describe accomplishes. If one of the integrals represents time, and the other work done in an engine, then the curve gives the continuous value of the horse-power per hour; or if one integral represents turns of a dynamo-machine, while the other represents electric current or electric energy, then the curve gives current-quantity or current-energy per turn. Or, generally, if two things are turning, either or both at a variable rate, a dividing machine will give the ever-varying value of the quotient of one by the other.

I have made use of two principles in the construction of dividing machines, which may therefore be classed under two heads. In the first class a pointer, if at a wrong position on the scale of quotients, moves towards its right place with a speed proportional to its distance from it: its motion is therefore of a logarithmic nature. In machines of the second class a pointer, if at a wrong position on the scale of quotients, changes its speed of moving towards its right position with a speed proportional to its distance from it: its motion therefore is of an harmonic nature. In either case the movement of the pointer may be made to trace a diagram on a travelling band of paper. I shall describe one logarithmic, and three harmonic dividers.

Logarithmic Divider.

For this a pair of wheels incapable of steering are required—that is, wheels which when turned while their edges are in contact with a surface are compelled to move forward, but which at the same time are perfectly free to move laterally. Disks with smoothly milled edges very imperfectly fulfil these conditions; but an outside smoke-ring, such as is described in the smoke-ring integrator, *anteà*, p. 10, should answer this purpose well. Let two such smoke-rings be mounted on a common axis, but so that each may revolve independently of the other; let there be a disk so supported that its plane is parallel to the common axis, and so that its own axis would if continued meet the other at a point midway between the two smoke-rings; moreover, let the axis of the disk be carried by an arm in such a manner that it is capable of moving in a direction parallel to the plane of the disk, but inclined at a small angle to the common axis of the smoke-rings. Fig. 1 shows the arrangement in its central position: A A is the common axis, $S\ S_1$ are the smoke-rings, and D the disk; the dotted line shows the line of travel of the axis of the disk. If while the disk is in its central position the two rings $S\ S_1$ are caused to revolve at equal speeds in opposite directions, then the disk merely turns, and there is no further result. Let the direction of motion be that shown by the arrow.

Now let the ring S_1 begin to revolve faster than S, then the centre of the disk D would, if free, begin to move downwards with a speed equal to half the excess; but it is incapable of moving vertically downwards; yet it may move down the slope indicated by the dotted line. Now, as the rings $S\ S_1$ in no way interfere with the lateral movement of the disk, its centre will move down the slope till it reaches such a point that the ratio of the distances of the point from the two rings is equal to the ratio of the speeds. It will be necessary to show that what is true when the produced axis of D intersects A A, is equally true when it has so far moved up or down the slope as to cause a considerable displacement. Let the centre have moved down to c, fig. 2, and let $S\ S_1$ be the points on the disk touched by the two smoke-rings, and let $c\,p$ be the perpendicular on $S\ S_1$ from c. Now, if $S\ S_1$ are turning with

speeds proportional to the lengths $S p$ and $S_1 p$, then the centre will have no tendency to move up or down. The motion of S about c as a centre may be resolved into two:—one, an upward motion, proportional to $S p$ (and this motion must, from the mechanical construction, be the same in the disk and in the ring); and the other, a lateral motion, proportional to $p c$, with which the ring in no way interferes. In a similar way, the motion of S_1 may be resolved into two—a downward motion proportional to $S_1 p$, and a lateral motion proportional to $p c$. The downward motion must be common to the disk and ring, while the lateral motion is free. It is clear then, if the rings move with speeds in the ratio $S p : S_1 p$, that the centre c will have no up or down tendency; and if the rings are not moving with speeds in the ratio $S p : S_1 p$, that the centre c will move up or down the slope with a vertical speed proportional to the distance of the point p from a point which does divide $S S_1$ in the ratio of the speeds of the smoke-rings. If θ is the inclination of the dotted line, then c will move along this line with a speed equal to cosec θ times its vertical speed, and p will travel along $S S_1$ with a speed equal to cot θ times the vertical speed of c. Should one disk ever stop and change the direction of its motion, then c must move along the slope till it is immediately under the ring, and move beyond till it arrives at such a position c_1 that $S p_1 : S_1 p_1$ is the ratio of the speeds.

Let the two smoke-rings be turned by two integrating machines as already described, then either or both may be going at a variable speed. There must at every moment be some point x in the line $S S_1$ such that $S x : S_1 x$ is the ratio of the speeds. This point will sometimes coincide with P, at which times C will be stationary; it will generally, however, be distant more or less from P, in which case P will pursue it wi h a speed proportional to its distance from it. If the arm which carries C carries also a pencil bearing against a uniformly travelling band of paper, then the curved line drawn will show what has been the efficiency, horse-power per hour, or whatever it was set to find during any period of time. The travelling band would have to be ruled across with lines showing time, and longitudinally with lines showing ratios, the scale being of the kind shown in fig. 3.

If the slope or the direction of motion had been in the opposite direction to that shown, then p, instead of approaching its places, would have fled from it with a speed proportional to its distance from it.

I think it possible that the logarithmic divider might be applied to solve some difficult problems; for while in action the inclination of the path of the centre c to the line $S S_1$, or the position of either ring on their common axis, may be changed in any way without interfering with the freedom of the motion of c.

Harmonic Dividers.

All harmonic dividers depend on the steering-power of wheels, an action which renders wheels of ordinary construction useless in the case of the logarithmic divider. The steering action, however, is not determined by any direct effect, as in the case of my cart integrating-machine, or in the more familiar case of a common bicycle, but depends on an intermediate action, and is in this respect exactly similar to the steering arrangements in that most beautiful and ingenious machine the "Otto" bicycle. In this the rider produces a difference in the speeds of two wheels, one on each side of him; the angular deviation therefore is the integral of the difference in speed between the two wheels, while the linear deviation from the original straight course is the integral with respect to the distance run of the sine of the angular deviation. The method of adapting this principle is exactly analogous to the case of the disk-cylinder integrator. That machine was developed in this way:—Take a cart integrator running on fixed ground, remove the whole of the cart except its steering-wheel, which fix in position, then give it movable and cylindrical ground to run on; a disk-cylinder integrator will be the result. In this case do away with the whole of Otto's machine except the two wheels, fix them in position, and give them each a movable floor to run on. Fig. 4 is a side view, and fig. 5 is a plan, of a machine of this kind. $A A_1$ are the two steering wheels mounted independently on a common axis; $B B_1$ are a pair of disks geared together by their edges, and they form the movable floors for the disks A A to rest upon. $c c_1$ are the supporting wheels of a frame F, and

are capable of running on rails parallel to the common axis of $A\,A_1$. The frame F carries a block D swivelled on a vertical axis passing through the centre of both F and D. The axes of $B\,B_1$ are supported by D. If $A\,A_1$ are caused to turn with speeds proportional to their distances from the centres of $B\,B_1$ and in the same direction, $B\,B_1$ will revolve at equal speeds in opposite directions, and neither D nor F will be affected; but if either A or A_1 is made to revolve a little faster than is due to its central distance, D will begin to turn in its swivel-frame. But no sooner does D begin to twist, than the disks $A\,A_1$, which previously were describing circles on $B\,B_1$, tend to move in spiral paths, the faster one receding from, and the slower one approaching, the centres of the disks $B\,B_1$ on which it is moving. But as $A\,A_1$ are incapable of any movement but rotation, while the disks $B\,B_1$ can, from the nature of their support, accommodate themselves to every kind of movement except one of lateral translation, therefore the combination c, F, D, B, and B_1 will move longitudinally till the central distances of $A\,A_1$ are proportional to their speeds; but, unfortunately, by this time the obliquity of the block D has become a maximum, and therefore its rate of longitudinal travel is a maximum also. It therefore travels on and introduces an error of position on the opposite side, which is corrected as before; and so the frame F oscillates on either side of its correct position. The motion may be considered more exactly in this way:—The rate at which D twists is proportional to the error of F, while the rate at which F corrects its position is proportional to the amount of twist of D: this expressed mathematically gives the equation $\frac{d^2y}{dx^2}=-y$, the solution of which is $y=\sin x$ or $y=\cos x$. The motion of both D and F therefore is harmonic. An exactly analogous case is that of a heavy body moving under the influence of a force which varies with the displacement, the movements of F being equivalent to that of the heavy body, while the rate at which D twists represents the force. Of course in the dividing-machine described we have nothing to do with inertia or with a resisting force; but the movements are the result of an action of pure rolling, and it so happens that they follow exactly the same law as the movements of a vibrating heavy body. This

view of the subject removes all difficulty in discovering what will be the action during either slow or very rapid variations in the ratio of the velocities. Let the dynamical equivalent be a heavy magnetic needle balanced in a magnetic field. Then the direction of the field corresponds to ratio of velocities, and the motions of the needle to the motion of the frame F. As has been shown in the case of a constant ratio, an error of position of F at starting causes F to oscillate on either side of its correct place, just as a displacement of a magnetic needle causes it to oscillate. Now suppose F to be in its correct position and then the ratio to change slowly (that is, slowly compared with the time of an oscillation); then the frame F will move slowly also, always being in the correct position, because during a slow change in the direction of the magnetic field a balanced needle follows the change without oscillation. Next suppose the ratio to alternate rapidly—that is, rapidly compared with the time of an oscillation, as, for instance, is the case when a steam-engine is made to do work uniformly, for work is put in intermittently and taken out gradually; then the dividing-machine will steadily show the mean value, and will take no account of the rapid variations in ratio, because a heavy magnetic needle subjected to rapidly alternating currents, which produce rapid variations in the direction of the magnetic field, is unaffected by these currents, but steadily maintains its position in the mean direction of the field. The most complex possible case is a combination of rapidly alternating and slowly changing ratios with occasional sudden changes. The first two are properly considered by the machine, while the sudden changes merely cause the frame F to oscillate on either side of its correct position. As in the case of the logarithmic divider, so here; a curve may be drawn on a travelling band of paper by a pencil attached to the frame.

A more simple arrangement is shown in fig. 6, where the disks B B_1 and the frame and block are replaced by a sphere S with its axis horizontal mounted in a horizontal ring R, and where this ring is supported in a crutch *c* which is capable of moving round on a vertical axis. Thus, when R is horizontal, the sphere can turn independently round three axes at right angles to one another. Then, if the disks A A_1 touch

the sphere in points 90° apart, the ratio of their speeds will be measured by the tangent of the inclination of R, while the deviation of *c* will correspond to the obliquity of the block D in the last machine.

The most simple harmonic divider that I can imagine is made by mounting, as shown in fig. 7, two iron cones with their bases adjacent, and with the lowest generating line in each horizontal and in one straight line. Then, if a magnetized steel reel is hung on and the two cones are turned, the reel will travel about and find the ever-varying value of the ratio of their speeds. The method of attaching a recording-pencil is too obvious to need description. If the two cones were placed so that their bases were turned away from one another instead of being adjacent, then the reel would change its speed of moving *away* from its correct position with a speed proportional to its distance from it. This kind of action could only exist, of course, for a very short time.

Whether dividing-machines are likely to be of general value for practical or experimental purposes, it is difficult to say; but there can be little doubt that cases might arise in which some machine such as I have described might be used with advantage.

IV. *On the Violet Phosphorescence in Calcium Sulphide. By* Captain W. de W. Abney, *F.R.S.**

In some investigations in photography it became necessary that I should study the phenomenon of phosphorescence exhibited in calcium sulphide, such as is employed in Balmain's paint. And as one or two points of interest arose which have not (as far as I am aware) been described before, I have thought it might be of interest to lay the subject before the Society.

The phosphorescent light, which is a peculiar violet, can be generated, if I may use the term, by day-light or candle-light —by the former fairly brightly, and by the latter only feebly, for reasons which will appear. In order to gain strong phosphorescence, the light from a magnesium-ribbon or the electric

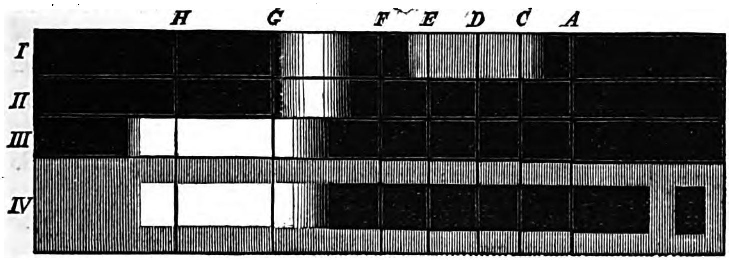
* Read January 28, 1882.

light should be employed. Mr. Warnerke has shown that $\frac{1}{8}$ inch of ribbon of the former is sufficient to excite phosphorescence to the maximum point that can be obtained from light of that brilliancy. Light of greater brightness, however, seems to excite it even more strongly. In a communication made to the Society by Lieut. Darwin, R.E., he gave the formula for the decrease of phosphorescence after excitation, from which it will be seen that it very rapidly diminishes in brilliancy.

My first experiment was to determine the spectrum of the emitted light; and this I observed with a small spectroscope; and the emission-spectrum is shown in I, figure opposite. It will be seen that to the eye the greatest luminosity is between G and F, and a feebler one extending from between E and F as far as the eye can recognize colour of low intensity towards the red. It became a matter of curiosity to know if any rays lay beyond the violet; and for this purpose an Iceland-spar prism and quartz lenses to the collimator and camera were brought into requisition, together with extremely sensitive photographic plates. Glass was spread with a layer of this sulphide and held together by paraffin as a substratum; the phosphorescence was excited by the electric light, and the tablet held in front of the slit. The exposure lasted one minute, when the phosphorescent tablet was again excited, and used as a source of light as before. After forty such excitations the photographic plate was developed; and a band (shown in II) was registered, which absolutely coincided with the band already registered in the visible spectrum. There was no trace of any radiation having wave-lengths in the ultra-violet. Whether there is any radiation below the red is a moot point; but from the gradually increasing brightness of the spectrum in the yellow, it seems probable that there is such.

The next point to ascertain was the part of the spectrum which excited phosphorescence. A tablet similarly prepared was exposed to the spectrum of the electric light, which showed carbon-bands strongly just above H, and also in the violet near G. A sensitive photographic plate was placed in contact with the tablet, and allowed to remain in contact 40 seconds, after which it was developed. III shows the locality of the spectrum by which phosphorescence was ex-

cited. This agreed absolutely with the visual observations. The exact locality was fixed by the carbon-bands above alluded



to, and also by comparing it with a sensitive photographic plate exposed in the ordinary manner. It will be noticed that the ultra-violet rays do not seem to cause phosphorescence in this case.

It now remained to register those rays which destroy the phosphorescence. This was effected in the following manner:—A tablet was first excited, and after half a minute exposed to the spectrum: those rays which destroyed phosphorescence were distinctly visible, as were also those which excited it. The tablet was placed in contact with a sensitive plate and developed. The wave-lengths were determined by placing a liquid in front of the slit and noting the place where the known absorption-bands in the infra-red region occurred, which is here shown by these localities remaining of the same luminosity on the tablet surrounding the impressed spectrum. The rays which destroy phosphorescence of this description are shown in IV. It will be noted that in the infra-red region is a portion which does not destroy it. When the wave-lengths of this are compared with the wave-lengths of the exciting portion about G and H, it is found that they are octaves one to another. (In the figure the infra-red region is much compressed, owing to the spectrum used being a prismatic spectrum.)

This fact appears remarkable and worthy of note. My own impression is that another band below this may also be traced; but as it was not shown upon the photographic plate, I have not mapped it. Such a band would probably be another octave below the second band. It should be noted that the infra-red band apparently is of the same luminosity as the general lumi-

nosity of the plate, and that these rays only feebly excite the plate.

I have endeavoured to make out any spectral difference in the light excited about H and about G, and have failed to obtain evidence as to any alteration in colour. It seems indifferent whether phosphorescence be excited by the indigo or by the violet rays.

I am at present engaged upon other phosphorescent material.

V. *On the Refractive Index and Specific Inductive Capacity of Transparent Insulating Media. By* J. Hopkinson, *D.Sc., F.R.S.**

One of the deductions from Maxwell's electromagnetic theory of light is, that the specific inductive capacity of a medium is equal to the square of its refractive index. Another deduction is, that a body which is opaque to light, or, more generally, to radiant energy, should be a conductor of electricity. The first deduction appeared so clear an issue that many experimenters have put it to the test. The results may be briefly summarized thus:—Some bodies (such, for example, as hydrocarbon oils† and paraffin-wax) agree with Maxwell's law so well that the coincidence cannot be attributed to chance, but certainly points to an element of truth in the theory: on the other hand, some bodies, such as glass‡ of various kinds, fluor-spar§, Iceland spar§, and the animal and vegetable oils‖, have specific inductive capacities much greater than is indicated by their refractive indices.

How do these latter results really bear on Maxwell's theory? The facts are these. Taking the case of one substance as typical, the refractive indices of light flint-glass are very accu-

* Read February 25, 1882.

† Silow, Pogg. *Ann.* 1875, p. 382; 1876, p. 306. Hopkinson, Phil. Trans. 1881, part i. p. 371.

‡ 'Cavendish Researches,' edited by Clark Maxwell; Schiller, Pogg. *Ann.* 1874, p. 535; Wullner, *Sitz. k. bayer. Akad.* 1877, p. 1; Hopkinson, Phil. Trans. 1878, part i., 1881, part ii.

§ Romich and Nowak, *Wiener Sitz.* Bd. lxx. part 2, p. 380.

‖ Hopkinson, Phil. Trans. 1881, part ii.

rately known, the period of disturbance ranging from $\frac{1}{4{\cdot}0 \times 10^{14}}$ second to $\frac{1}{7{\cdot}6 \times 10^{14}}$ second; the specific inductive capacity is known to be about 6·7, the time of electrical disturbance being from $\frac{1}{17000}$ second to a few seconds. If from the observed refractive indices we deduce by a formula of extrapolation the refractive index for very long waves, we find that its square is about one third of 6·7. There can be no question about the accuracy of the observed refractive indices; and I have myself no doubt about the specific inductive capacity; but formulæ of extrapolation are always dangerous when used far from the actual observations. If Maxwell's theory is true, light flint-glass should be perfectly transparent to radiations having a wave-period of, let us say, $\frac{1}{17000}$ second; because this glass is sensibly a perfect electrical insulator, its refractive index for such waves should be about 2·6. Are there any facts to induce us to think such a thing possible? It is well known that in some cases strong selective absorption of light in the visible spectrum causes what is known as anomalous dispersion; that is to say, the body which presents such selective absorption of certain rays has a refractive index abnormally low for waves a little shorter than those absorbed, and an index abnormally high for waves a little longer than those absorbed*.

Light flint-glass is very transparent through the whole visible spectrum, but it is by no means transparent in the infra-red. If the absorption in the infra-red causes in light flint-glass anomalous dispersion, we should find a diminished refractive index in the red. We may say that we have a hint of this; for if we represent the refractive indices by the ordinates of a curve in which the squares of the reciprocals of the wave-lengths are abscissæ, this curve presents a point of inflection†. In the part corresponding to short waves it is concave upwards; in the part corresponding to long waves it is concave downwards: the curvature, however, is very slight. Does it not seem possible, looking at the matter from the purely optical point of view, that if we could examine the spectrum below the absorption in the infra-red, we should find the effect of anomalous dispersion, and that the refractive index

* 'Theory of Sound,' by Lord Rayleigh, vol. i. p. 125.

† Proceedings of Royal Society, 1877.

of such long waves might even be so high as 2 6? To test this experimentally in a conclusive manner would probably not be easy. Perhaps the best chance of finding how these long waves are refracted would be to experiment on the rays from a thermopile to a freezing-mixture. Without an actual measurement of a refractive index below all strong absorption, it cannot be said that experiment is in contradiction to the Electromagnetic Theory of Light; for a strong absorption introduces a discontinuity into the spectrum which forbids us from using results on one side of that discontinuity to infer what they would be on the other side.

VI. *Water-pipes that do not burst with Frost.* *By* C. VERNON BOYS, *Demonstrator of Physics, Normal School of Science, South Kensington**.

DURING the severe weather of last winter, Mr. L. S. Powell proposed to me a scheme for preventing the possibility of water-pipes bursting through frost; and I have since learnt that Mr. Mangnall, of Manchester, independently hit upon the same idea. As far as I can remember, there were some letters in the 'Times' describing the use of india-rubber pipes containing air inserted in the service-pipes. This would obviously prevent pipes from bursting; for the pressure is of a nature that is relieved by a comparatively small expansion; and this the india-rubber tube allows to take place in the surrounding water when it collapses. There is, however, one serious objection to this, which is the possibility of the detachment of one end of the flexible tube, in which case a rush of water might cause it to accumulate in one place and obstruct the passage.

Mr. Powell's plan is to make the piping elliptical—either before it is laid, in which case it may be made of that form originally or by passing round pipe through rollers, or afterwards, when suitable hand-squeezers will effect the result without the necessity of removal. As will afterwards be seen, it is not necessary that the pipe should be elliptical throughout; if

* Read November 12, 1881.

left round under staples and in other inaccessible places, the adjacent elliptical portions ensure safety. The principle, of course, is obvious. As is well known, the Bourdon pressure-gauge depends on the fact that the area of an elliptical pipe is less than that of a circle of equal perimeter: therefore during increased pressure its section becomes more circular; increased circularity of section produces diminished curvature in the form of the pipe; and so the movements of the end of the pipe are used to measure pressure. Thin brass is used for this purpose, and is so elastic that it returns to its original form when the pressure is removed; and so an indefinite number of increments and decrements of pressure may be measured by it. The case of the elliptical water-piping is different. Here there is not a definite pressure to withstand, but a definite increase of volume; and, moreover, if this increase of volume is resisted, a practically infinite force arises to break down the resistance. The question then is, how best to allow of this increase of volume. The method of the indiarubber pipes I have already mentioned. The other plan is to make them of an elliptical or other-than-round section. There is, however, far more in this suggestion than one would be likely to see at first. Consider the case of a round pipe in which water is beginning to freeze. Increase of volume must take place somewhere. No pipe can be absolutely uniform in strength everywhere. So wherever a place occurs which happens to be a little weaker than the rest, no matter how little, that place will stretch, and necessarily stretch more than other places. But when a round pipe stretches, two things happen—its diameter increases and its thickness decreases; therefore, as the strength of a tube to resist bursting is inversely as the diameter and directly as the thickness, each of these effects makes the stretched portion still weaker than the neighbouring parts; therefore a round pipe under the action of frost is in a state of unstable equilibrium; the consequence is, knobs form on the pipe, and ultimately burst.

Now consider the case of an elliptical pipe, of such strength of course, as to stand the ordinary water-pressure. As before suppose some portions are weaker than others. When expansion takes place they will suffer most, and will begin to give way. But an elliptical pipe on giving becomes more circular

and this the more easily as its section departs more from the circle; so the very fact of its becoming more circular makes it less ready to change its form. In a very little while, therefore, though originally weaker, it will become as strong as neighbouring portions: therefore an elliptical pipe under the action of frost is in a state of stable equilibrium, and, instead of giving way in the bulbous manner of a circular pipe, it uniformly becomes more circular. Now the expansion of water in becoming ice is known; and therefore it is easy to calculate by compound interest how many *complete* freezings (that is, freezings from one end of the pipe to the other) any given section of piping will stand before it becomes round. Of course, in practice, the whole length of a pipe does not get frozen; yet if it were originally all of it elliptical, the unfrozen portions would be effective in preventing the more exposed parts from bursting; because as soon as the exposed portions have become rounder than the rest, the latter, and not the former, will yield. If two places in the pipe become completely frozen through and then the intermediate portion freezes, it is true that parts beyond the frozen plug will have no effect.

Mr. Powell and I tried a series of experiments on the subject to see if, in practice, the pipes behaved as we expected. We obtained a quantity of $\frac{3}{4}$-inch lead pipe, about $\frac{1}{10}$ inch thick, and some thin composition pipe of the same size. The piping was cut into lengths of about three feet; half of them were squeezed into an approximately elliptical form, and the rest left circular in section. The degree of ellipticity was such that the major axis was a little more than twice the minor axis. One end of each pipe was squeezed together and soldered. Into the other ends brass plugs cut with a sharp thread were screwed while hot, having been previously smeared with a cement of rosin, beeswax, and red-ochre. After these plugs were inserted and while still hot, the lead was, as an additional precaution, squeezed over a narrower portion of the plug above the screw-thread. In each plug a hole had been drilled and tapped. Through these holes the pipes were filled with water; and then iron screws, with washers of leather boiled in beeswax and tallow, were used to make a tight joint. The pipes were then all laid together in a long box, and surrounded with

a freezing-mixture. When a short test-pipe of the same diameter showed that the water was completely frozen, the pipes were removed and thawed. The round composition pipe was burst. The round lead pipe was swollen in an irregular manner. The elliptical piping had become slightly rounder, but was perfectly uniform in shape from end to end, which was not the case when it was put in the freezing-mixture. The most noticeable thing, however, was the fact that all the unburst pipes had become good water-hammers, and this showed that leakage could not have occurred. The screws were removed, the pipes filled, again screwed up and refrozen, and this was repeated till all were burst. The round lead and the thin elliptical composition pipe burst at the third freezing, and the elliptical lead pipe at the sixth. Judging from the fact that it required three freezings to burst the round pipe, one might be led to suppose that a round pipe would last equally long under ordinary conditions, which is certainly not the case. The reason is that, under ordinary conditions, a greater length freezes at a time and more slowly; and a slight inequality arising, the expansion from a greater volume of water is concentrated on the weaker places, which therefore give way during the second, if not the first, freezing of the water.

It might be thought that, as the outer layers of ice are below the freezing-point when the pipe is being cooled, they would not act as a plastic body and accommodate themselves to the changing form of the pipe; but no doubt can remain as to their behaviour in this respect when the pipe is cooled in air; for in the freezing-mixture, where the rate of cooling must be much more rapid, such accommodation takes place perfectly, even thin composition pipes changing their form and becoming round. The apparent plasticity of the ice may depend on fracture and regelation; for if the outer layers are below the freezing-point, and a bursting-pressure is brought to bear on the compound pipe formed of lead and ice, it might yield, the lead bending and the ice cracking, and so allowing the water to penetrate the cracks and freeze in them. Whether this action takes place or not does not much matter: the result, as in the somewhat different case of glacier-motion, is much the same.

If the pipe is made of such a form that it will not become round till it has been completely frozen, say, three times, it will take a great many frosts to burst it, as those parts that do not freeze easily will protect the more exposed portions; so absolute security may be relied upon till ordinary round pipes have burst once or twice; and then the now nearly round ones may be squeezed back to their original form. The choice, then, is between two evils; either burst pipes, with the usual damage and cost of repair, or the trouble of inspection every second or third time that the neighbours find that the "thaw" has burst theirs.

We thought it possible that iron might be sufficiently elastic to return to its original form; and so we froze water in two $\frac{3}{4}$-inch iron gas-pipes, one round and one which had been flattened when red-hot. The round pipe burst the first time; the flat one did return slightly when thawed, but not enough to prevent its bursting during the second operation.

No doubt most people will not consider this proposal of Mr. Powell's a satisfactory cure for burst pipes; they would like something which could be fixed in their houses and which would be always safe without further attention. But till such a discovery is made, I think elliptical pipes give the best solution of a problem which has troubled every householder.

As the subject of this paper is of physical as well as general interest, I hope that it may be considered not unworthy of the attention of the Physical Society.

VII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part V. *By* C. R. ALDER WRIGHT, *D.Sc.* (*Lond.*), *F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School**.

On the Relationships between the Electromotive Force of a Daniell Cell and the Chemical Affinities involved in its Action.

102. IN accordance with the theorem stated in § 61, the E.M.F. that would be requisite to break up a given electrolyte under given conditions into the "nascent" products of electrolysis would be a constant amount, were it not that

* Read February 11, 1882.

the secondary physical and chemical actions of the electrodes, and of dissolved gases, &c. upon the nascent products give rise to the development of an amount of heat, the energy equivalent to which diminishes the work that would otherwise be done by the current whilst effecting electrolysis; so that the E.M.F. corresponding to the net electrolytic work actually done is less than the constant amount that would be requisite in the absence of these interfering circumstances; under certain conditions, the diminution in the work is so great that work is gained instead of spent, when the cell becomes an electromotor. Experiment shows that, *cæteris paribus*, the amount of diminution is less the more rapid the rate of current-flow; so that in a decomposing cell, in which, on the whole, work is spent during the passage of the current in doing electrolysis (the heating effect due, in accordance with Joule's law, to the resistance proper of the cell being left out of consideration), the counter E.M.F. set up (representing the work so spent) is of + sign, and increases in magnitude with the rate of current-flow; whilst in an electromotor, in which, on the whole, work is gained during the passage of the current, the counter E.M.F. set up is of − sign (*i. e.* is a direct E.M.F.), and decreases in magnitude with the rate of current-flow.

This decrease in magnitude, although a phenomenon well known under the name of "polarization of the cell," has nevertheless been less thoroughly investigated than is desirable. Thus, for instance, in the case of a given Daniell cell it is unknown to what relative extents the diminution is due to each of three entirely different possible causes, viz.:—first, the formation, in consequence of the electrolytic actions going on, of a stronger zinc-sulphate solution round the zinc plate, and of a weaker copper-sulphate solution round the copper plate, than were there originally; secondly, the more or less incomplete action as regards setting up E.M.F. of the energy gained by the solution of the zinc, and displacement thereby of copper from the copper-sulphate solution; and, thirdly, the somewhat analogous want of completeness in transformation into E.M.F. (and quantity of electricity jointly) of the energy gained by the transformation into ordinary copper of the nascent metal thus set free. In order to refer briefly to this possible want

of completeness in development of E.M.F., it will be convenient to term that portion of the energy due to the various actions taking place in the cell that does contribute to the setting-up of difference of potential, the "adjuvant" portion of this energy; whilst the remainder is spoken of as the "non-adjuvant" energy. Of course the non-adjuvant energy in practice makes its appearance in the form of heat developed *ab initio*, and not in accordance with Joule's law—*i. e.* not due simply to the passage of a current through a resistance.

As regards the possible non-adjuvancy of energy thus indicated, it is to be noticed that whilst the observations of numerous experimenters agree in showing that, under certain conditions, at least an approximate equality subsists between the electromotive forces actually developed in a Daniell cell, and in various analogously constructed cells, and those corresponding to the net chemical changes taking place therein (viz., in the case of a Daniell cell, the displacement of copper from copper sulphate by zinc), this approximate equality does not exist under all conditions even in a Daniell cell, inasmuch as, first, considerable discrepancies exist between the values obtained by different observers working under different conditions, and, secondly, the same cell exhibits values varying with the rate of current-flow through so-called "polarization;" whilst, on the other hand, with certain forms of cell the maximum E.M.F. developed falls considerably short of that corresponding to the net chemical action. These discrepancies and amounts of falling short appear to be in certain cases considerably greater than can be accounted for by the formation of solutions of zinc and copper sulphates &c. of different densities through "migration of the ions," thus indicating considerable extents of non-adjuvancy.

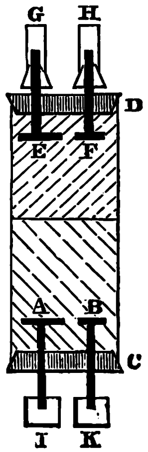
In order to obtain further information upon these points, a large number of observations have been made upon various forms of Daniell cell and allied combinations; the general results of which are that, with the normal Daniell combination (zinc, zinc sulphate, or dilute sulphuric acid, copper sulphate, copper), the amount of non-adjuvant energy with suitable plate-surfaces and with feeble rates of current-flow is insensible, but becomes very considerable with more rapid rates of flow, or with certain impure forms of metallic plate-surfaces—the non-adjuvancy

being partly due to incomplete development as E.M.F. of the energy due to the solution of the zinc, but more especially to the imperfect development as E.M.F. of that due to the transformation of nascent into ordinary copper; whilst the formation of solutions of densities different from those of the fluids originally employed also contributes to the diminution in the effective E.M.F. of the cell. With certain other forms of cell, more or less non-adjuvancy exists under all circumstances. In the following paper only those experiments referring to the normal Daniell cell are described, the remainder being postponed to a later occasion.

Experiments made to determine the total fall in E.M.F. through so-called Polarization, occurring in variously arranged Daniell Cells for definite amounts of increase in the rates of Current-flow.

103. A gravity Daniell cell was constructed (fig. 1) with two zinc plates, A and B, supported so that their upper surfaces were in the same horizontal plane, by means of stout wires passing through an indiarubber cork, C, the under surfaces of the plates, and the wires between the cork and plates, being covered with gutta percha. This cork fitted into the lower end of a wide glass tube some 4 or 5 centim. in diameter and 12 or 15 long; at the other end was a precisely similar cork arrangement, D, carrying two copper plates, E and F, of which only the lower surfaces were uncovered with gutta percha. The plates A and E were each of such size as to expose precisely 2·5 square centim. of surface, whilst the plates B and F each exposed double that area. In order to arrange the cell as a gravity battery, a concentrated zinc-sulphate solution (sp. gr. nearly 1·4) was run into the cell until half full, and then a cold-saturated solution of copper sulphate (sp. gr. somewhat below 1·2) was carefully floated on the top of the zinc-sulphate solution. In some experiments this disposition of the plates was reversed, the copper plates being lowest and the zinc plates highest; in these cases saturated copper-sul-

Fig. 1.



phate solution was first poured in, and then a lighter zinc-sulphate solution. To avoid the almost inevitable contamination of the zinc-sulphate solution with faint traces of copper sulphate which occurs when the former is poured on to the latter, no matter how carefully done, it was found more satisfactory to fill the cell half full of the zinc-sulphate solution, and then slowly to introduce the copper sulphate at the base of the cell through a U-tube passing through the cork C, the end inside the cell being drawn out to a point and bent downwards, so that the copper-sulphate solution flowed in gradually and lifted up the zinc-sulphate solution without passing into it as a jet and so more or less impregnating it with copper.

It is evident that, since A and B, E and F are respectively in the same horizontal planes, the resistance of the column of fluid between the plates, R, must be sensibly the same, whether the smaller pair of plates, B and F, or the larger pair, A and E, be employed to generate a current; either of which can at pleasure be done by simply connecting the mercury-cups G H I K, attached to the appropriate plates, with the extremities of a known external resistance. By measuring the difference of potential E subsisting between the ends of this resistance the current passing, C, is known; hence if R be known, the E.M.F. of the cell, e, is known, being given by the equation

$$e = \mathrm{E} + \mathrm{CR}.$$

The value of R can be deduced, with a fair amount of precision, from the results of two series of observations with varying currents, made, first, with the smaller, and, secondly, with the larger pair of plates, in the following way:—By dividing the actual current-strengths by 2·5 and 5·0 (the superficies in square centimetres of the plates respectively), two series of values of E for corresponding "current-densities" (rates of flow per square centimetre of plate surface) are obtained, by interpolation from which, for a given current-density D, two values, E_1 and E_2, are deduced for the smaller and the larger pair of plates respectively. The E.M.F. with the smaller pair, e_1, is manifestly

$$e_1 = \mathrm{E}_1 + 2{\cdot}5\,\mathrm{DR},$$

whilst that with the larger pair is

$$e_2 = \mathrm{E}_2 + 5{\cdot}0\,\mathrm{DR}.$$

It is evident that, when the density of the current is the same, and the surfaces of the two copper and of the two zinc plates are respectively in the same conditions, the effect on the E.M.F. caused by the passage of a current must be sensibly the same, inasmuch as the same amount of zinc is dissolved and of sulphate of zinc formed, and the same quantity of copper is deposited and of copper sulphate decomposed, per square centimetre of plate surface; so that e_1 must sensibly $=e_2$.

Hence, since

$$E_1+2{\cdot}5\,CR=E_2+5{\cdot}0\,CR,$$

it results that

$$R=\frac{E_1-E_2}{2{\cdot}5\,D}.$$

By contrasting in this way the values of the E's obtained with various current-densities, a set of values for R are obtained, fairly concordant when the observations are carefully made and the plates of such materials as to remain in the same condition of surface throughout the experiment, or nearly so, so that on introducing a given external resistance into the circuit, sensibly the same values for the E's are uniformly obtained. With sulphate-of-zinc solution surrounding the zinc plates, and with amalgamated plates (copper as well as zinc), this permanence is more readily ensured than when dilute sulphuric acid is used (with amalgamated zinc plates), or when the copper plate is not amalgamated, but only freshly coated with electro-deposited metal. Indeed, to obtain a sufficient number of readings when dilute sulphuric acid is employed, it is preferable to discharge the cell after an hour's use and recharge it, amalgamating the plates afresh, and filling up with the same solutions as before to exactly the same levels (ensured by suitably marking the glass), so that the resistance of the cell may vary as little as possible, the temperature being so adjusted as to be sensibly the same on the average throughout. Thus the following series of values was obtained as the average result of four sets of readings alternately with gradually increasing and gradually diminishing external resistances, with a cell containing nearly saturated copper-sulphate solution, freshly electro-coated copper plates, amalgamated zinc plates, and dilute sulphuric acid of 1·045 sp. gr., the plates being reamalgamated and reelectro-coated respectively for each successive series—all the observa-

tions being reduced to the same standard as that adopted throughout this paper, viz. the average reading at 15·5 of a large number of Clark's cells taken as 1·457 volt*.

Smaller plates.			Larger plates.		
C_1.	D_1.	E_1.	C_2.	D_2.	E_2.
·001738	·0006952	·869	·00385	·000770	·770
·000960	·0003840	·960	·001854	·0003708	·927
·000509	·0002036	1·018	·000999	·0001998	·999
·0002112	·00008448	1·056	·0005195	·0001039	1·039
·0001075	·00004300	1·075	·0002136	·00004262	1·068
·0000543	·00002172	1·085	·0001085	·00002170	1·085
·0000273	·00001092	1·092	·0000546	·00001092	1·092
·00001057	·00000423	1·097	·0000262	·00000524	1·097
·00000533	·00000213	1·100	·0000108	·00000216	1·100
·00000215	·00000086	1·103	·00000535	·00000107	1·103
·00000107	·00000043	1·103	·00000215	·00000043	1·103
·00000052	·00000021	1·103	·00000105	·00000021	1·103

* The exactness of this value depends not only on how far the average of these cells is identical with the average of those which served as the basis of Clark's valuation (Proc. Roy. Soc. xx. p. 444), but also on the exactness with which the B.A. unit of resistance is determined. If this latter be too small—as appears probable from the experiments of Joule, and of the writer and Mr. Rennie (Phil. Mag. March 1881, p. 169), from the results of Rowland, and from the recent experiments of Lord Rayleigh and Prof. Schuster—the true value of an average Clark's cell is below 1·457 to the same proportionate extent: thus, if the B.A. unit be really 0·99 $\frac{\text{earth-quadrant}}{\text{second}}$, the E.M.F. of an average Clark's cell is only $0·99 \times 1·457 \times 10^8 = 1·442 \times 10^8$ C.G.S. units. In view, however, of the fact that the question of the amount and even of the direction of the error (if any) in the B.A. resistance unit is not yet absolutely settled, it is assumed in this paper that there is no error at all.

For analogous reasons the value of J is assumed, as previously, to be 42×10^6 ergs, the evidence in support of its having a higher value still being not inconsiderable; although the probability is that, if the B.A. resistance unit be only 0·99 $\frac{\text{earth-quadrant}}{\text{second}}$, J is close to $41·5 \times 10^6$. The value of χ (the electrochemical constant defined in § 7) deduced in § 9 as the most probable, viz. ·000105, is also adhered to, notwithstanding that Mascart's recent experiments (*Comptes Rendus*, xciii. p. 50) tend to indicate that this value is too large, ·0001044 representing his final result: this value is 0·8 per cent. lower than ·00010527, the mean value deduced from Kohlrausch's experiments. If J be taken $= 41·5 \times 10^6$, and χ be assumed $=$ ·0001048 (mean of Mascart and Kohlrausch's results), the value of χJ, the factor for reducing gram degrees to C.G.S. E.M.F. units, becomes

104. From these figures the following values for R are deduced—the first four determinations only of each series being employed, on account of the smallness of the differences between E_1 and E_2 in the other cases:—

D.	E_1.		E_2.		E_1-E_2.	$R=\frac{E_1-E_2}{2\cdot5\times D}$.
	Observed.	Interpolated.	Observed.	Interpolated.		
·000770	...	·847	·770	...	·077	4·00 ohms.
·0006953	·869	...	...	·799	·070	4·03 „
·0003840	·960	...	...	·921	·039	4·05 „
·0003708	...	·965	·927	...	·038	4·11 „
·0002036	1·018	...	...	·997	·021	4·12 „
·0001998	...	1·019	·999	...	·020	4·04 „
·0001039	...	1·050	1·039	...	·011	4·24 „
·0000845	1·056	...	...	1·047	·009	4·28 „
					Average	4·11 ohms.

Taking 4·11 ohms as the average value of R, the following numbers are calculated from the above observations, the values of E_1 and E_2 being obtained by interpolation:—

Values from observations with smaller plates.				Values from observations with larger plates.			Average value of E.M.F. of cell.	Fall.
D.	E_1.	2·5×DR.	$e_1=E_1+2\cdot5DR$.	E_2.	5·0×DR.	$e_2=E_2+5\cdot0DR$.		
0	1·103	...	1·103	1·103	...	1·103	1·103	0
·000001	1·103	...	1·103	1·103	...	1·103	1·103	0
·000002	1·100	...	1.100	1·100	...	1·100	1·100	·003
·000005	1·096	...	1·096	1·097	·001	1·098	1·097	·006
·00001	1·092	·001	1·093	1·092	·002	1·094	1·094	·009
·00002	1·086	·002	1·088	1·086	·004	1·090	1·089	·014
·00005	1·072	·005	1·077	1·065	·010	1·075	1·076	·027
·0001	1·051	·010	1·061	1·041	·021	1·062	1·062	·041
·0002	1·019	·021	1·040	·999	·041	1·040	1·040	·063
·0004	·957	·041	·998	·916	·082	·998	·998	·105
·0007	·868	·072	·940	·798	·144	·942	·941	·162

It is evident that the values of e_1 and e_2 accord so closely

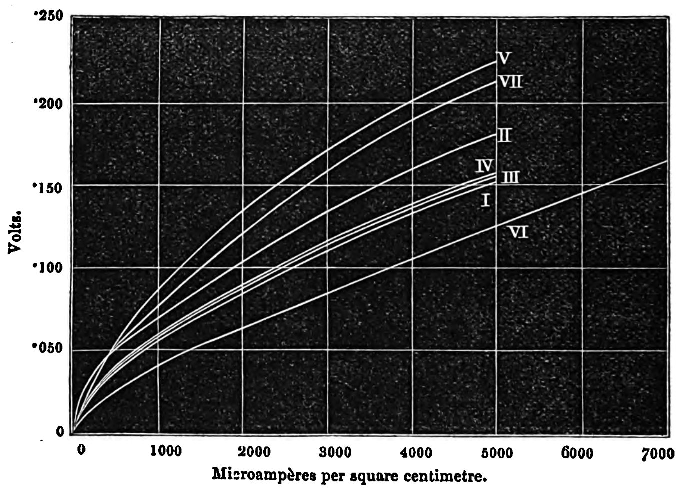
4349, or 1·4 per cent. less than 4410, the value hitherto assumed, and still retained in this paper.

Prof. S. P. Thompson applies the term "Faraday coefficient" to the numerical value χ (Journ. Soc. Arts, xxx. p. 34); should this term be generally accepted, the letter F might gracefully be used instead of χ to indicate the factor, just as J is used to indicate the Joule coefficient.

that their average may fairly be taken as representing, with but little error, the E.M.F. of a cell containing copper and zinc plates in the condition of those experimented with in this instance*.

105. In various other analogous experiments the concordance was usually not quite so close as in this example, a smaller number of readings (one or two sets only) being made; but the discrepancy was in no case so great as materially to influence the general character of the curve representing the variation in the value of the E.M.F. with varying currents, obtained on plotting the results by making the currents abscissæ and the electromotive forces ordinates. The following table (p. 53) exhibits, side by side, the values obtained in various acses selected as specimens: in numerous other cases not quoted, the curves obtained were considerably similar to those indicated by the figures given in the table and represented in fig. 2. The

Fig. 2.



resistance of the cell, R, is stated in ohms, and the electromotive forces in volts; the "maximum E.M.F." indicates the

* It might appear at first sight that a third valuation of the E.M.F. of the cell might be deduced as follows:—Since $e_1=E_1+2{\cdot}5\,DR$, and also $e_2=E_2+5{\cdot}0\,DR$, it results that $e_1=e_2=2E_1-E_2$; but it is evident that this value is such that e_1 is the arithmetical mean between e_2 and $2E_1-E_2$, and hence that the average of the three values must be identical with e_1, and therefore less exact than the mean of e_1 and e_2.

		I.	II.	III.	IV.	V.	VI.	VII.
Zinc plates		Pure metal scraped bright.	Bright pure metal.	Commercial zinc plate scraped bright.	Coated over with a film of copper by immersion in weak copper-sulphate solution.	Pure metal amalgamated.	Pure metal amalgamated.	Pure metal amalgamated.
Copper plates		Freshly electro-covered.	Freshly electro-covered.	Freshly electro-covered.	Freshly electro-covered.	Electro-metal thoroughly amalgamated.	Freshly electro-covered.	Electro-metal thoroughly amalgamated.
Fluid surrounding zincs		Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·10.	Zinc-sulphate solution, sp. gr. 1·10.	Zinc-sulphate solution, sp. gr. 1·42.	Dilute sulphuric acid, sp. gr. 1·045.	Dilute sulphuric acid, sp. gr. 1·045.
R		11·2	13·0	9·5	5·4	11·15	4·11	4·2
Maximum E.M.F.		1·098	1·093	1·091	1·050	1·094	1·103	1·096
Current-density.		Observed amounts of Fall in Electromotive Force.						
C.G.S. units.	Microampères.							
·000001	10	...	...	·001	·001	...	0	...
·000002	20	...	...	·006	·006	...	·003	...
·000005	50	·009	·010	·013	·011	·005	·006	·008
·00001	100	·018	·021	·019	·018	·011	·009	·016
·00002	200	·025	·032	·028	·028	·023	·014	·028
·00005	500	·038	·049	·044	·046	·048	·027	·050
·0001	1000	·052	·069	·056	·060	·083	·041	·082
·0002	2000	·082	·101	·085	·087	·134	·063	·120
·0003	3000	·106	·130	·107	·110	·174	·084	·160
·0005	5000	·149	·179	·151	·155	·223	·125	·212
·0007	7000	...	...	...	...	...	·162	...

average value of the highest E.M.F. observed throughout the various series of readings, this value being always observed when either no current at all circulated, or a current of less magnitude than about 8 microampères* per square centimetre. The copper plates were uniformly surrounded by nearly saturated copper-sulphate solution (sp. gr. 1·175); the zinc plates were sometimes surrounded by nearly saturated zinc-sulphate solution (sp. gr. about 1·4), and were then lowest; in other cases they were highest, and were then surrounded either by zinc-sulphate solution of sp. gr. 1·10, or by dilute sulphuric acid of sp. gr. 1·045.

It is noticeable that whilst experiments Nos. I. and II. show that the curves obtained are by no means necessarily identical even when the conditions are sensibly the same (owing apparently to differences in the character of the copper deposited during the action of the cell), experiments Nos. I., III., and IV. indicate that but little difference in the curve is brought about by using commercial instead of pure zinc, or by altering the surface of the zinc by covering the bright metal with a film of copper (although more or less marked depressions in the maximum E.M.F. are occasioned thereby); on the other hand, experiments V. and VII., as compared with the others, indicate that amalgamating the copper renders the rate of fall in E.M.F. sensibly more rapid. But little difference, on the whole, is apparently occasioned in the curves by the use of zinc-sulphate solution (whether stronger or weaker than the copper-sulphate solution), as compared with dilute sulphuric acid; what difference is brought about is of this kind—that the sulphuric-acid curves slightly underlie the zinc-sulphate curves.

In none of the experiments made was any measurable depression of the E.M.F. of the cell brought about when the current flowed at a rate not exceeding 8 microampères per square centimetre; and in several cases this rate of flow might be doubled before any depression greater than ·001 volt (0·1

* In accordance with the nomenclature adopted by the recent International Electrical Congress, the term *ampère* is used throughout this paper to indicate what in the former paper of this series was designated a *weber*, viz. 0·1 C.G.S. current-unit; so that a microampère = ·0000001 C.G.S. current-unit = 10^{-7} ampère.

per cent.) was occasioned. As a rule, when the current-density was from 30 to 50 microampères per square centimetre, a diminution in the E.M.F. of from 0·5 to 1 per cent. was brought about; whilst diminutions of 10 per cent. and upwards were occasioned when the current-density exceeded 3000. Supposing the same values to hold for ordinary Daniell cells (which is probably not quite the case, as the zinc and copper plates are usually unequal in size), it results that, with cells of ordinary dimensions (*e. g.* holding a litre and exposing a surface of 500 square centimetres), no appreciable diminution in the E.M.F. would be brought about when the current does not exceed $500 \times 8 = 4000$ microampères (·004 ampère); whilst diminutions of several tenths per cent. would be occasioned with currents of fivefold magnitude (·02 ampère), and diminutions of 10 per cent. and upwards when the current exceeds $500 \times 3000 = 1{,}500{,}000$ microampères (1·5 ampère).

Experiments made to determine the maximum Electromotive Forces of variously arranged Daniell Cells containing Zinc-sulphate solution around the zinc, and the maximum proportion of the Fall in the E.M.F. of the Cell with gradually increasing currents, that could be due to accumulation round the plates of fluids of different densities through the migrations of the ions.

106. A long series of experiments was next made with the object of determining how far the very considerable diminution in the E.M.F. of a Daniell cell, above shown to exist when moderately strong currents are generated, can be accounted for by the strengthening of the solution of zinc surrounding the plate, and the weakening of the copper-sulphate solution round the copper plate, which necessarily take place in consequence of the migration of the ions accompanying the passage of the current. Inasmuch as the use of dilute sulphuric acid introduces complications, these observations were made in the first instance with zinc-sulphate solutions only round the zinc plates; later on (§ 111), similar experiments with cells containing sulphuric acid are described.

It results from the experiments of Moser (*Annalen der Physik*, iii. p. 216) and H. F. Weber (Phil. Mag. [5] viii. pp. 487 &

523), that when a stronger solution of zinc (or copper) sulphate diffuses into a weaker one of the same salt, plates of zinc (or copper) placed in the two solutions acquire different potentials, that in the stronger solution being at the higher potential: the potential difference reckoned per a constant difference in specific gravity of solution (*e. g.* a difference of 0·1) is not constant, but depends on the actual values of the specific gravity, being less the stronger the solutions. The maximum value obtained (in the case of zinc-sulphate solutions containing respectively 60 and 1 per cent. of crystallized salt) was only ·036 Daniell, or about ·040 volt; whence it would seem that if the effect produced by zinc-sulphate solution in diffusing into copper-sulphate solution is of the same order of magnitude as that produced by diffusing into another zinc-sulphate solution of strength equivalent to that of the copper-sulphate solution, the effect on the E.M.F. of a Daniell cell, due to migration of the ions, cannot possibly materially exceed ·04 volt; whilst, whatever the magnitude of the effect, it must tend to diminish the E.M.F. of the cell, since it partially equalizes the difference of potential between the zinc and copper plates set up by the chemical action alone. In order to find out the actual magnitude of the diminution due to this diffusive action in various cases, a number of determinations were first made of the E.M.F. set up in various forms of Daniell cell when generating currents of magnitude not exceeding 8 microampères per square centimetre (and usually when generating no current at all), *the zinc- and copper-sulphate solutions being in any given case of the same specific gravity**. The average values being thus fixed, the observations were then repeated, using solutions not of the same specific gravity. It is particularly noteworthy in this connexion, that the E.M.F.

* Although two solutions of zinc and copper sulphate, respectively of the same specific gravity, are not absolutely chemically equivalent to one another (*i. e.* do not contain precisely equivalent percentages of the two salts), yet the difference in specific gravity between any two solutions of equivalent strengths is so small that, for all practical purposes, it may be assumed that when the specific gravity is the same the solutions are of equivalent strengths. Direct determinations of the specific gravities of various solutions of equivalent strengths (made by dissolving known weights of the air-dry pure salts to known weights of aqueous solutions), gave the following results:—

of a Daniell cell was found to be *sensibly independent of the strength of the solutions when both are of the same specific gravity*—*i. e.* the deviations observed from equality were less than the experimental errors.

The cells employed were constructed of two small beakers —one containing zinc-sulphate solution and plates of zinc (scraped bright, covered with electro-deposited metal, or amalgamated with pure mercury), and the other copper-sulphate solution and similar copper plates. The two beakers were connected, in the way described by Raoult (*Ann. de Chim. et de Phys.* 4th series, ii. p. 317, and iv. p. 392), by means of an inverted Y-tube, the ends of which, dipping into the two beakers respectively, were covered over with thin bladder, the tube being filled with the zinc-sulphate solution. Each one of the zinc and copper plates used was soldered to a platinum wire fused into a piece of glass tubing, forming a mercury-cup; the soldering and the whole length of the platinum wire being thickly covered with gutta percha, so that only zinc (or copper) was exposed to the fluid. The plates were then connected with a series of mercury-cups in such a way that, by simply moving a double switch connected at one end with two mercury-cups in connexion with the electrometer-quadrants, and dipping at the other end into two of the series of cups, any required pair of zinc and copper plates could be brought

Percentage of $CuSO_4, 5H_2O$.	Specific gravity at 18°.	Equivalent percentage of $ZnSO_4, 7H_2O$.	Specific gravity at 18°.
1	1·006	1·15	1·007
3·75	1·023	4·3	1·026
7·5	1·047	8·6	1·053
15	1·098	17·2	1·107
22·5	1·156	25·9	1·163
30	1·214	34·5	1·223

The 30-per-cent. solution of copper sulphate was slightly supersaturated, and deposited crystals on standing in a closed vessel; saturated zinc-sulphate solution has a specific gravity upwards of 1·4. It is noteworthy that these figures indicate that when given bulks of water and of either zinc- or copper-sulphate solution are mixed, an *increase* in bulk occurs, thus agreeing with J. Thomsen's result that dilution of a solution of either salt is accompanied by heat-absorption (§ 113).

into connexion with the electrometer. Fig. 3 shows the arrangement used for two pairs of plates, and fig. 4 that for

Fig. 3.

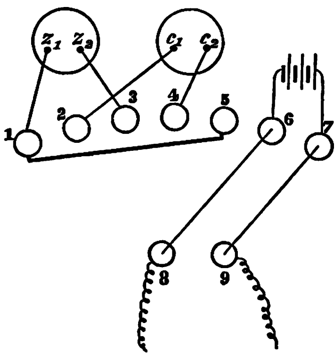
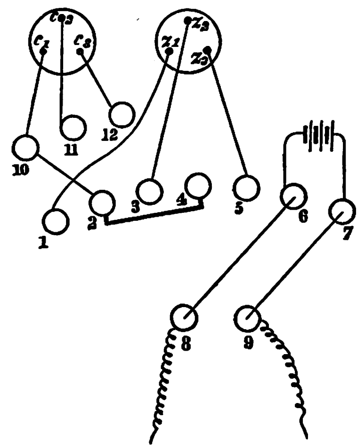


Fig. 4.



three pairs. In the former the two zinc plates, z_1 and z_2, are connected with mercury-cups Nos. 1 and 3 (No. 1 being also connected with No. 5), whilst the two copper plates, c_1 and c_2, are connected with cups Nos. 2 and 4 respectively. Cups Nos. 6 and 7 are connected with a standard cell (the error of which, in reference to the average taken as 1·457 volt, is known). By means of a double switch any pair of the series of cups 1 to 7 can be connected with cups 8 and 9, which are connected with the electrometer through the usual reversing-gear; so that when Nos. 1 and 2, 2 and 3, 3 and 4, and 4 and 5 are thus connected, the electromotive forces due to the combinations z_1c_1, c_1z_2, z_2c_2, c_2z_1 are respectively read off; whilst when 6 and 7 are connected (as represented in the figure) the electrometer-scale is standardized. In actually taking readings a double set was always employed, the switch being successively used to connect the four combinations and the standard with the electrometer, and then to connect them again in reversed order; so that, by taking the averages of the two sets, any error due to running down of the electrometer during the readings might be eliminated (in practice the running down during the period was insensible, the variation being usually at most only 2 or 3 per cent. during the whole

day, and often much less). When three pairs of plates were used, the three zincs were connected respectively with cups 1, 3, and 5; whilst cup No. 2 was connected with No. 4, and also, by means of a movable wire, with either of three other cups, Nos. 10, 11, and 12, with which respectively the three copper plates were connected: so that when No. 2 was connected with 10, as represented in the figure, the combinations c_1z_1, c_1z_2, and c_1z_3 could be read off by connecting the double switch with 1 and 2, 2 and 3 (or 3 and 4), and 4 and 5 successively; and similarly for the other combinations.

107. The ultimate results of upwards of a hundred series of valuations of the electromotive forces of various combinations, mostly lasting over four hours, were as follows:—

(1) With the stronger solutions used (specific gravities 1·100 to 1·175) the E.M.F. set up after the first few minutes remained sensibly constant for several hours (the temperature being constant), never differing from the final average of the four average sets of readings made in each of the first four hours by amounts outside the limits of observational error. For instance, the following values were obtained in one experiment, in which the temperature throughout was close to 18°, the specific gravity of the solutions being 1·175:—

Combination.	Average E.M.F. determined during the				Final average.
	1st hour.	2nd hour.	3rd hour.	4th hour.	
Amalgamated zinc—electro-copper ...	1·112	1·113	1·114	1·114	1·1132
Bright zinc—electro-copper	1·110	1·109	1·112	1·110	1·1102
Amalgamated zinc—bright copper ...	1·119	1·122	1·122	1·120	1·1207
Bright zinc—bright copper	1·117	1·115	1·118	1·118	1·1170

Very similar results were obtained in all the other cases. After twenty-four hours the E.M.F. usually diminished to a greater or less extent. These changes are referred to later on (§ 108), being probably due to oxidation of the metals by dissolved air.

(2) With weaker solutions (sp. gr. 1·0065 to 1·050) the E.M.F developed during the first half hour or so was usually

slightly lower than the value attained subsequently; which value remained sensibly constant for several hours, and then fell to a greater or lesser extent, as with the stronger solutions. Accordingly, in such cases the lower readings during the first half hour or so were not taken into account in the final average. For any given combination of plates, the final average thus obtained with weaker solutions was sensibly identical with that obtained with the stronger solutions*.

(3) The combinations that gave the most constant results on repetition of the experiments were those containing amalgamated zinc and either electro-copper or amalgamated copper; next to which were those with electro-deposited zinc and these same kinds of copper plates. Combinations containing either bright zinc or bright copper (*i. e.* rods of fused metal or sheets of rolled metal filed, scraped, or sand-papered to

* The conclusion that the E.M.F. developed by a given pair of plates immersed, the one in zinc-sulphate, the other in copper-sulphate solution, is sensibly independent of the strength of the solutions when both are of the same density (or, at least, that the variation in E.M.F. caused by variation in strength of the solutions is not outside the limits of experimental error), is further corroborated by the results of still more direct experiments on the matter. Three cells were arranged, containing solutions respectively of the specific gravities 1·010, 1·090, and 1·175, each containing a recently electro-coppered plate and a freshly amalgamated zinc plate. A number of readings were taken of the potential-differences subsisting between the plates in each case; and then the plates were exchanged—the pair from the first cell being placed in the second, that originally in the second being transferred to the third, and so on. After a new set of readings had been taken, the plates were again exchanged and a third set of readings taken; so that each pair of plates was read in each cell. The following figures were finally obtained, all readings taken during the first half hours after immersion of the plates being rejected:—

Specific gravity 1·010.		Specific gravity 1·090.		Specific gravity 1·175.	
1st pair of plates	1·1127	2nd pair of plates	1·1122	3rd pair of plates	1·1125
2nd „ „	1·1130	3rd „ „	1·1115	1st „ „	1·1118
3rd „ „	1·1140	1st „ „	1·1135	2nd „ „	1·1133
Mean	1·1132	Mean	1·1124	Mean	1·1125

In each case the value found as the mean for the three pairs of plates differs from the general average 1·1127 by an amount so small as hardly, if at all, to be outside the limits of experimental error.

perfect brightness) exhibited a considerably wider range of variation. By comparing various bright copper plates with one and the same amalgamated zinc plate, or various bright zinc plates with one and the same electro-copper plate, it was found that differences, amounting nearly to 0·010 volt in the most extreme instances, were observable in each case with different bright plates as compared with one another. On the other hand, on comparing various electro-copper or amalgamated copper plates with one and the same zinc plate, or various amalgamated zinc plates with one and the same copper plate, the extreme ranges of fluctuation were found to be not more than half those observed with bright plates, and usually did not exceed $\pm$·001 as compared with the average.

(4) As the ultimate average result of all the determinations made, it was found that an amalgamated zinc plate gave, when opposed to a given copper plate, an E.M.F. lower by ·002 volt than a bright zinc plate, and lower by ·001 volt than an electro-zinc plate. The actual differences in various experiments ranged in the former case between +·006 and −·004, the bright zinc plate sometimes giving a higher value, and sometimes a lower value, than the amalgamated plate—more frequently the former. (In the example quoted above, the bright zinc plate gave a value lower by ·0030 when opposed to an electro-copper plate, and by ·0037 when opposed to a bright copper plate.) With electro-zinc as compared with amalgamated zinc, the difference ranged between +·004 and −·003, the electro-zinc sometimes giving a higher and sometimes a lower value than the amalgamated zinc, more usually the former.

(5) Similarly, the effect of substituting a bright copper plate for an electro one in any given combination was uniformly to cause an increase in the E.M.F. by an amount varying between ·001 and ·010 volt, and averaging, on the whole, ·006. (In the example quoted above, bright copper gives a higher value than electro-copper by ·0075 when opposed to amalgamated zinc, and by ·0068 when opposed to bright zinc.) The effect of amalgamating a copper plate was found to be, on the whole, to give an E.M.F. lower by ·001 than that given under the same conditions by a freshly electro-coppered plate, the actual difference ranging between +·002 and −·003, the amalga-

mated plate sometimes giving a higher value and sometimes a lower one than the electro-plate, more frequently the latter. It is worthy of notice that no sensible difference was observable whether the plate amalgamated were previously of bright rolled metal or of electro-metal; so that, on the whole, the effect of amalgamating a bright rolled plate was to depress the E.M.F. by ·006 + ·001 = ·007.

(6) The average results of all the experiments made are contained in the following tabular statement: increase in E.M.F. is indicated by the + sign, and diminution by the − sign:—

Variation in the E.M.F. due to the use of zinc- and copper-sulphate solutions of different strengths between the limits of sp. gr. 1·0065 and 1·175, both solutions being of the same specific gravity in any given case . . .	Less than ±·001.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for fresh electro-copper :—				
Bright rolled copper sheet	+·010	+·001	·009	+·006
Amalgamated copper (surface wet with fluid mercury)	+·002	−·003	·005	−·001
Effect of substituting for fresh electro-zinc :—				
Bright cast zinc	+·005	−·005	·010	+·001
Amalgamated zinc (surface wet with fluid mercury)	+·003	−·004	·007	−·001
Electromotive force of combinations :—				
Amalgamated zinc—Amalgamated copper	1·115	1·110	·005	1·113
„ „ Electro-copper	1·116	1·111	·005	1·114
„ „ Bright copper	1·124	1·115	·009	1·120
Electro-zinc—Amalgamated copper	1·116	1·110	·006	1·114
„ „ Electro-copper	1·118	1·111	·007	1·115
„ „ Bright copper	1·124	1·114	·010	1·121
Bright zinc—Amalgamated copper	1·119	1·109	·010	1·115
„ „ Electro-copper	1·121	1·110	·011	1·116
„ „ Bright copper	1·126	1·112	·014	1·122

108. The above figures are, as already stated, derived from the observations made during the first four hours after setting up the cells, the values registered during the first half hour or so with weaker solutions being rejected on account of their frequently being lower than the sensibly constant values attained to

subsequently. When the cells were allowed to stand for twenty-four hours, or for longer periods, a greater or less fall in the E.M.F. was usually noticed: by taking out any pair of the plates (*e. g.* the amalgamated zinc and the electro-copper plates) and replacing them by a freshly prepared similar pair, it was found that the value rose again to sensibly the same value as on the previous day when only set up a few hours; so that by taking out first one and then the other of the pair of plates, determinations could be made of the amount of the total fall attributable to alterations of either plate separately. The actual values thus obtained fluctuated considerably. As a general rule, it was found that bright copper plates gave the same value after twenty-four hours' immersion as they did at first; but occasionally the value was lowered by ·002 to ·004. Electro-copper plates usually gave values less by ·002 or ·003 after twenty-four hours than at first; and in some instances, when the pink electro-metal was sensibly browned or discoloured by oxidation, either all over or here and there in spots, the diminution was even greater, sometimes as much as ·010. Amalgamated copper plates, if the surface were still white and brilliant after twenty-four hours, gave the same value as at first; but if the mercury had sunk into the copper, and brown spots of oxidized metal were here and there visible, the E.M.F. was a few thousandths of a volt lower than at first. With the zinc plates greater diminutions were, as a rule, observed. In some cases amalgamated plates showed little or no diminution after twenty-four hours; but generally a diminution of ·002 to ·005 was observed; whilst with bright and electro-zinc plates diminutions of from ·001 to ·015 were noticed. On the whole, after twenty-four hours the E.M.F. was sometimes unchanged, and sometimes less by ·020. After forty-eight hours the fall was more perceptible still, the few combinations that had not appreciably altered during twenty-four hours always showing a decided fall after a longer period. It is noticeable in this connexion, that cells after Daniell's construction, but containing other metals than copper, did not always give the same results as normal Daniell cells. Thus, for instance, whilst cells containing cadmium sulphate and cadmium plates behaved like copper Daniells, in that the E.M.F. was sensibly steady for some hours after first setting up, and only exhibited a mea-

surable fall after several hours had elapsed, and not always then, analogous cells containing silver sulphate and silver plates invariably showed a perceptible fall in less than an hour after first setting up, the diminution becoming progressively greater as a longer time elapsed. That this diminution was due to a change (presumably oxidation by dissolved air) induced on the surface of the zinc plate was rendered evident by the fact that, on taking out from such a zinc-silver cell the zinc plate after the lapse of an hour or more, and opposing it to electro-copper in an ordinary zinc-copper Daniell, an E.M.F. was indicated considerably less than the value given by a fresh zinc plate, and usually just about as much less as represented the fall in E.M.F. observed with the zinc-silver cell at the end of the period during which it was observed, as compared with the E.M.F. at the beginning of that period, when it was newly set up.

109. It is further to be noticed, that all the above-mentioned figures were obtained with cells the nature of the construction of which was such that diffusion of copper-sulphate into the zinc-sulphate solution, and consequent deposition of copper on the surface of the zinc, *did not take place at all* during the whole time that the observations lasted. With ordinary gravity-cells it is almost impracticable to prevent traces of copper reaching the zinc after some twenty-four hours at latest: the effect of the deposition of even the faintest traces of copper on the zinc surface is to cause a considerable fraction of the energy due to the solution of the zinc to become non-adjuvant, and thus materially to diminish the E.M.F. Thus, for instance, the following figures were obtained with one cell, and similar ones in numerous other cases:—

E.M.F. of gravity-cell newly set up: zinc plate wholly free from copper	1·103
After 8 hours: faint tarnish visible on the zinc .	1·095
„ 24 hours: slight film of copper on the zinc .	1·070
„ 48 hours: thick film of copper on the zinc .	1·045

In much the same way, the presence of even small quantities of impurities in the zinc causes an appreciable diminution in the E.M.F. In all the above-described observations, some of the purest zinc that could be bought was employed, being

fashioned into plates and rods by melting in a porcelain crucible, pouring out on a fire-clay tile, and cutting into slips with a chisel, &c. When commercial sheet or cast zinc was used, or when pure zinc was amalgamated with imperfectly purified mercury, the E.M.F. developed when such zinc was opposed to a given copper plate was often very materially less than the value obtained with pure zinc, or pure zinc and pure mercury. Thus, whilst values varying between 1·111 and 1·116 were obtained with pure amalgamated zinc opposed to fresh electro-copper as above described, values varying from 1·080 to 1·109 were obtained with commercial zinc amalgamated with pure mercury, and with pure zinc amalgamated with impure mercury, similarly opposed.

It is abundantly evident from the above-described results (not to speak of those detailed later on), that "the E.M.F. of a Daniell cell" is a unit of comparison subject to decidedly wide limits of fluctuation; but that it is possible to reproduce a standard cell of the kind within a maximum limit of variation of about $\pm 0{\cdot}25$ per. cent., by using Raoult's form of construction together with a recently electro-coppered or amalgamated copper plate, and a pure zinc plate amalgamated with pure mercury, the zinc- and copper-sulphate solutions used being both of the same specific gravity, the precise value of the specific gravity being immaterial. Even when made, however, such a standard cell cannot be relied on for more than a few hours. It will be shown in a subsequent paper that whilst Latimer Clark's mercurous-sulphate cell is subject to an even wider range of fluctuation in E.M.F. than the best forms of Daniell cell, its permanence is very far superior, a well-constructed cell giving absolutely the same value (when used in conjunction with a quadrant electrometer only) for months and months together.

110. The above described experiments having given results indicating the average values of the electromotive forces developed with different characters of plates when the specific gravities of the solutions surrounding the plates are the same, further series of observations were made with cells in which the zinc- and copper-sulphate solutions were not of the same

specific gravity, the mode of operating being otherwise the same as before. As predicable from Moser's figures, and as previously observed by H. F. Weber, it was found that when the copper-sulphate solution was the more dense of the two, the E.M.F. was higher than with solutions of equal density, and *vice versâ* when the zinc-sulphate solution was the stronger. The average results of various observations, mostly lasting over four hours as before, are contained in the following table; in every case the zinc plate employed was of pure metal and amalgamated with pure mercury.

Effect of increasing the strength of the Zinc-sulphate solution relatively to that of the Copper-sulphate.

Specific gravity of solution used.		Nature of copper plate.	Average E.M.F. at 18° of combination.			Ratio of difference in E.M.F. to difference in specific gravity.
Zinc sulphate.	Copper sulphate.		Observed.	Previously found for solutions of equal specific gravity.	Difference.	
1·145	1·010	Electro.	1·099	1·114	·015	
		Bright.	1·107	1·120	·013	
					·014	$\frac{·014}{·135}$ = ·105
1·175	1·050	Electro.	1·102	1·114	·012	
		Bright.	1·108	1·120	·012	
					·012	$\frac{·012}{·125}$ = ·096
1·260	1·050	Electro.	1·096	1·114	·018	
		Bright.	1·103	1·120	·017	
					·0175	$\frac{·0175}{·021}$ = ·083
*1·395	1·010	Electro.	1·077	1·114	·037	
		Bright.	1·086	1·120	·034	
					·0355	$\frac{·0355}{·0385}$ = ·092
*1·395	*1·175	Electro.	1·097	1·114	·017	
		Bright.	1·104	1·120	·016	
					·0165	·22 = ·075

Effect of increasing the strength of the Copper-sulphate solution relatively to that of the Zinc-sulphate.

1·010	1·090	Electro.	1·129	1·114	·015	
		Bright.	1·135	1·120	·015	
					·015	$\frac{·01}{·080}$ =0·188
1·010	*1·175	Electro.	1·132	1·114	·018	
		Bright.	1·141	1·120	·021	
					·0195	$\frac{·0195}{·165}$ = ·118
1·050	*1·175	Electro.	1·126	1·114	·012	
		Bright.	1·133	1·120	·013	
					·0125	$\frac{·0125}{·125}$ =·100
1·090	*1·175	Electro.	1·123	1·114	·009	
		Bright.	1·128	1·120	·008	
					·0085	$\frac{·0085}{·0085}$ = ·100

It is evident from these figures that the accumulation round the zinc plate of zinc sulphate, and the exhaustion of the copper sulphate in the solution round the copper plate, even if carried out to the utmost possible extent, could not diminish the E.M.F. of a Daniell cell by more than ·03 to ·04 volt; whilst it is hardly probable, even with tolerably rapid currents, that the accumulation would suffice to diminish the E.M.F. by more than half that amount—a diminution almost negligible in comparison with the much larger amounts, 0·1 volt and upwards, found above to be due to this cause of diminution and non-adjuvancy jointly.

It is noticeable in passing that the above figures show that when two solutions of zinc and copper sulphates interdiffuse, the E.M.F. set up (like that produced by the interdiffusion of two zinc-sulphate, or of two copper-sulphate solutions, as studied by Moser) is of such a character that the stronger solution acquires the higher potential; the actual value of the E.M.F. developed also *is less for a given difference in specific gravity the stronger the solutions*, and, so far as the two sets of results can be compared, agrees fairly with the values deducible from Moser's experiments—indicating that the difference between the E.M.F. set up when two different solutions of $\left\{\begin{array}{l}\text{zinc}\\\text{copper}\end{array}\right.$

* Solution almost saturated at ordinary temperatures.

sulphate diffuse into a constant $\left.\begin{matrix}\text{copper}\\ \text{zinc}\end{matrix}\right\}$ sulphate solution is substantially the same as the E.M.F. set up when these two different $\left.\begin{matrix}\text{zinc}\\ \text{copper}\end{matrix}\right\}$ sulphate solutions diffuse into each other. Moreover the effect of a given alteration in the strength of the zinc-sulphate solution (every thing else remaining the same) is sensibly equal in amount, but opposite in sign, to that of a similar alteration in the strength of the copper-sulphate solution; it is evident that only when this is the case can the E.M.F. of the cell be the same whether the solutions be strong or weak. It will be shown in a future paper that this property, though not absolutely peculiar to the normal Daniell cell, is still rather the exception than the rule with analogous voltaic combinations.

Experiments with Daniell Cells containing dilute Sulphuric Acid.

111. In all the above described experiments, the solution surrounding the zinc plate was one of pure zinc sulphate. Various previous experimenters, notably H. F. Weber, have found higher values for the electromotive forces of Daniell cells containing dilute sulphuric acid than for those containing zinc-sulphate solution (*vide* § 113): *à priori* a higher value might be anticipated, because a notable amount of heat is evolved on further diluting even weak sulphuric acid; so that the total energy gained in the cell is not merely that due to the displacement of copper from copper sulphate by zinc, but also that gained in the dilution of the sulphuric acid through the diffusion which necessarily goes on. On trying experiments of the same kind as those just described with cells containing dilute sulphuric acid of various strengths (the zinc being amalgamated), it was found that not only was there a considerable want of permanence in the E.M.F. set up, the values perceptibly decreasing after a period of time (varying in different cases from half an hour to several hours) had elapsed since setting up the cells, but, further, that two cells, apparently set up in identically the same way, exhibited much greater differences in their readings during the period before the E.M.F. began to diminish, than were observed in the zinc-sulphate cells examined as above described. On the whole,

however, the average values obtained distinctly pointed to the conclusion that, when the acid and copper-sulphate solutions are of the same specific gravity, the E.M.F. rises with strength of the solution ; and that when they are not of the same specific gravity (the acid not being stronger than specific gravity 1·18), the E.M.F. is sensibly that due to a cell containing liquids both of specific gravity equal to that of the acid in the cell examined, corrected by the addition (or subtraction) of a quantity representing the difference in specific gravity of the solutions multiplied by the numerical value deduced from the zinc-sulphate cell experiments, representing the difference in E.M.F. produced by a variation in the specific gravity of the copper-sulphate solutions equal to that between the acid and copper sulphate in the cell examined; *i. e*, if, for example, the E.M.F. of a cell containing both liquids of sp. gr. 1·100 be E, that of a copper-cell containing acid of sp. gr. 1·100 and copper-sulphate solution of sp. gr. 1·010 will be $E-(1{\cdot}100-1{\cdot}010)\times\alpha$, where α is the factor expressing the diminution per unit difference of specific gravity in the E.M.F. between the limits of sp. gr. 1·010 and 1·100 for copper-sulphate solution. Similarly, that of a cell containing acid of sp. gr. 1·100 and copper-sulphate solution of sp. gr. 1·175 would be $E+(1{\cdot}175-1{\cdot}100)\beta$, where β is the corresponding factor for a difference in specific gravity between the limits 1·100 and 1·175.

Thus, for instance, the following values were obtained with a cell containing fluids both of sp. gr. 1·175, the temperature being close to 18° throughout.

Period since setting up.	During 1st hour.	2nd hour.	3rd hour.	4th hour.	Average.
Electro-copper plate	1·161	1·162	1·163	1·160	1·1615
Bright " "	1·167	1·168	1·168	1·166	1·1672

In most cases, however, a distinct fall of upwards of ·005 volt occurred in less than four hours.

A number of similar series of observations (upwards of thirty) with various other cells, in which the fluids were always of equal specific gravity, gave the following results, the observations being only continued as long as the E.M.F.

remained sensibly constant—*i. e.* for a period of time varying from thirty minutes to four hours, and averaging about two hours. In all these experiments electro-copper and amalgamated pure zinc plates were employed.

Specific gravity of fluids.	Electromotive force set up, in volts.		
	Maximum.	Minimum.	Average.
1·010	1·143	1·121	1·129
1·050	1·150	1·128	1·139
1·090	1·155	1·137	1·148
1·175	1·179	1·161	1·169

Evidently, even with the weakest acid, the E.M.F. is sensibly above that developed with zinc-sulphate cells—viz. 1·114; whilst with stronger solutions the difference is yet more marked.

112. A number of analogous observations were made with cells containing dilute sulphuric-acid and copper-sulphate solutions, not both of the same specific gravity: the average results were as follows:—

Specific gravity of solutions.		Excess of specific gravity of copper sulphate over acid solution.	Approximate correction for excess of specific gravity of copper sulphate.	Average E.M.F. observed.	Observed E.M.F. corrected for excess of specific gravity of copper sulphate.
Acid.	Copper sulphate.				
1·010	1·050	+·040	−0·008	1·147	1·139
1·010	1·090	+·080	−0·015	1·135	1·120
1·010	1·175	+·165	−0·019	1·137	1·118
				Average......	= 1·126
1·050	1·175	+·125	−0·012	1·148	1·136
1·175	1·050	−·125	+0·012	1·168	1·180
1·175	1·090	−·085	+0·008	1·164	1·170
				Average......	= 1·175

The final averages representing the E.M.F. corrected to the uniform specific gravities 1·010, 1·050, and 1·175 respectively, do not differ from the values directly obtained as just described for these specific gravities by amounts outside the limits of experimental error in this class of the various experiments made.

A peculiar result was obtained with cells containing sulphuric acid of sp. gr. 1·265, and nearly saturated copper-sulphate solution of sp. gr. 1·175. The E.M.F. was considerably depreciated, the average value in four sets of experiments with electro-copper and amalgamated pure zinc plates being only 1·084 (maximum 1·095, minimum 1·067). On standing a few hours, copper-sulphate crystals formed at the junction of the two fluids, showing a much less degree of solubility of the salt in the acid fluid formed than in pure water.

Relationships between the maximum E.M.F. developed by a Daniell Cell and the Energy due to the net Chemical action taking place therein.

113. The above-described results afford a ready explanation of the discrepancies between the valuations of the E.M.F. of a Daniell cell that have been made in absolute measure by various observers, amongst the more important of which may be cited those of Bosscha (Pogg. *Ann.* ci. p. 517, 1856), von Waltenhofen (Pogg. *Ann.* cxxxiii. p. 478, 1868), Kohlrausch (Pogg. *Ann.* cxli. p. 456, and *Ergänz.* vi. p. 35), H. F. Weber (Phil. Mag. 1878, [5] v. p. 189), and J. Thomsen (Wied. *Ann.* xi. p. 246, 1880), all of which valuations lie between 1·088 and 1·132 volt when reduced to that unit (and, in the case of Bosscha's results, corrected for an error of about 8 per cent. in the value of the coil used by him). To these may be further added the electrostatic valuations of Sir W. Thomson and Latimer Clark, both of which lie near to 1·11 volt. Favre (*Comptes Rendus*, lxix. p. 35) and Raoult (*Ann. Chem. Phys.* [4] ii. p. 338, and iv. p. 392) obtained by methods involving calorimetric measurements numbers representing the "galvanic heat" of a Daniell cell, and equivalent to considerably smaller electromotive forces, their valuations (23993 and 23900 gramme-degrees respectively) corresponding to 1·058 and 1·054 volt. In these instances, and in the case of the lower values obtained by other observers, doubtless the "polarizations" produced by the passage of the tolerably powerful currents employed were considerable. The highest values were obtained with cells in which dilute sulphuric acid was used; thus, H. F. Weber found that a perceptibly higher value was obtained with such a cell than with one containing zinc-sulphate solution, viz. 1·1317 and 1·1286 (mean = 1·1301)

as compared with 1·0954. That this should be the case is predicable from the nature of the heat-evolutions taking place when zinc is dissolved in acid of various strengths. Let an amount of heat, H_1, be evolved when a gramme-equivalent of zinc oxide is dissolved in sulphuric acid of given strength, SO_4H_2, m H_2O; and let H_2 be the heat evolved on its solution in acid of a different strength SO_4H_2, n H_2O, n being less than m. Let the solution SO_4Zn, nH_2O, resulting in this latter case, evolve h_1 of heat on the addition of $(m-n)$ H_2O, so as to form the solution SO_4Zn, m H_2O; and let the heat evolved on the addition of this quantity of water to SO_4H_2, n H_2O, so as to convert it into SO_4H_2, m H_2O, be h_2. Then, if the zinc oxide were dissolved in the stronger acid, and the zinc sulphate diluted, the heat-evolution would be H_2+h_1; whilst if the acid were diluted first, and the zinc oxide were then dissolved in it, the total heat evolved would be H_1+h_2. Since of necessity the two amounts of heat, H_2+h_1 and H_1+h_2, must be equal, it results that $H_2=H_1-h_1+h_2$. Now h_2 is a considerable positive quantity in all cases; whilst Thomsen's results on the heat evolved on solution of salts in water (*Deut. chem. Ges. Berichte*, 1873, p. 710) indicate that when the solution is accompanied by heat-absorption (as is the case with zinc sulphate), a further heat-absorption takes place on diluting a stronger solution of the salt with water, so that h_1 has a negative value. Hence, on both accounts, H_2 is greater than H_1; that is, the work gained in the synthesis ZnO, SO_3 x aq. increases as x diminishes. Since the net chemical action in a Daniell cell is equivalent to the result of the actions $(Zn, O)+(ZnO, SO_3\ x\ \text{aq.})-(Cu, O, SO_3\ y\ \text{aq.})$, it finally results that the chemical action taking place in the cell develops an amount of energy which increases, *cæteris paribus*, as x diminishes, *i. e.* is greater the more concentrated the acid surrounding the zinc.

On the other hand, when the zinc plate is surrounded by zinc-sulphate solution instead of sulphuric acid, the effect of variation in the strengths of the copper- and zinc-sulphate solutions will be comparatively but small when both are of the same degree of molecular concentration (which, as shown above, is very nearly the case when they are of the same density). If SO_4Zn, nH_2O evolves, as before, h_1 on addi-

tion of $(m-n)H_2O$, and SO_4Cu, nH_2O evolves h_3 on a similar addition, and if H_3 and H_4 are respectively the heats evolved when zinc displaces copper from $Cu SO_4, nH_2O$, and $Cu SO_4, mH_2O$, it results that, if zinc displace copper from the stronger solution and the resulting $SO_4 Zn, nH_2O$ be diluted to $Zn SO_4, mH_2O$, the heat evolved will be H_3+h_1; whilst if the copper-sulphate solution be first diluted and then the zinc displaces the copper, the heat-evolution is H_4+h_3. Since, of necessity, $H_3+h_1=H_4+h_3$, it follows that $H_3=H_4+h_3-h_1$. Now, since the solution of zinc and copper sulphates (crystallized) is in each case accompanied by heat-absorption, it results that h_3 and h_1 are both negative, and hence that h_3-h_1 is negligible if h_3 is any thing like comparable with h_1 in magnitude; so that in this case the energy developed by the net chemical action taking place in a Daniell cell must be practically independent of the degree of concentration of the solutions.

114. The earlier calculations of J. Thomsen, referred to in §16, as to the heat evolved in the displacement of copper from copper sulphate by zinc, are for various reasons probably less accurate than the later results obtained by him (*Journ. prak. Chem.* [2] xi. p. 412, and xii. p. 271); these different values may be thus contrasted, the values being gramme-degrees per gramme-molecule:—

Values from experiments by Andrews, Dulong, Hess, Favre and Silbermann, and J. Thomsen.		Values from later experiments of J. Thomsen.
Zn, O, SO_3 aq. =	108460	106090
Cu, O, SO_3 aq. =	56216	55960
Difference =	52244	50130
Corresponding in volts (per gramme-equivalent) to	1·152	.... 1·105

The earlier value is deduced from observations in which the heat of formation of copper oxide from the metal by combustion is involved, the copper being in a more or less *compact* state, filings &c.; the latter involves the determination of the heat-evolution during the precipitation of *spongy* copper from copper sulphate by iron. Leaving out of sight other sources of difference between the two values, this affords a reason why the former value should be the higher, since heat is evolved in the transformation of spongy into compact

copper *. On the whole, it is evident that the net chemical change taking place in a Daniell cell (*i. e.* the displacement of copper from copper sulphate by zinc) corresponds to an E.M.F. which is a little higher than 1·105 volt by an amount which is the greater the more compact the copper precipitated, and is approximately constant when the zinc plate is surrounded by zinc-sulphate solution of the same equivalent strength as the copper-sulphate solution surrounding the copper plate, but is influenced by the strength of the dilute sulphuric acid when the fluid surrounds the zinc plate. The amount of this influence can be approximately calculated from Thomsen's determinations of the heat developed in the formation of the solutions of strengths indicated by H_2SO_4, nH_2O where n varies (*Deut. chem. Ges. Berichte*, iii. p. 496). Thomsen finds in gramme-degrees per gramme molecule:—

n	gramme-degrees.	n	gramme-degrees.
9	14940	199	17056
19	16248	399	17304
49	16676	799	17632
99	16850	1599	17848

From which table the values for any intermediate values of n can be obtained by interpolation. In the experiments leading to the valuatiou 50130 gramme-degrees for the heat developed during the precipitation of a gramme molecule of copper by zinc, Thomsen used fluids containing altogether 800 molecules of water to one of zinc sulphate, &c. Hence, were the acid used in the synthesis Zn, O, SO_3 aq., to be H_2SO_4,

* That this is so is shown by the circumstance that if a current be sent through a decomposing-cell containing copper-sulphate solution and copper electrodes, of which the positive one is of compact rolled metal, a considerably higher difference of potential is set up, under any given conditions and with a steady current, than is set up when the + electrode is replaced by one covered with freshly electro-deposited metal. The more spongy texture of the latter corresponds to a greater heat-development during solution than that taking place with the compact metal, and hence to a diminution of the work that has to be done by the current in passing; with not very powerful currents the difference often exceeds ·02 or ·03 volt, corresponding to 450 to 700 gramme-degrees per gramme equivalent. In a somewhat similar way, but using the mercurial calorimeter, Favre found (*Comptes Rendus*, lxxiii. p. 1258) that electro-copper gave out about 1000 gramme-degrees more heat than rolled metal per gramme equivalent; this would correspond to an E.M.F. of ·044 volt.

17 H_2O (corresponding nearly to the sp. gr. 1·175), instead of H_2SO_4, 800 H_2O, the heat-development would be greater than 106090 by 17632−15986=1646 gramme-degrees per gramme molecule (15986 being the heat of dilution of sulphuric acid when $n=17$, deduced from the above table): this corresponds to 823 gramme-degrees per gramme equivalent =·036 volt; *i. e.* the E.M.F. corresponding to the heat-development during the displacement of copper from copper sulphate by zinc would be greater than 1·105 volt by ·036, or would be 1·141 volt. To this amount should also be added the value of the E.M.F. equivalent to the heat-absorption during the dilution of $ZnSO_4$, 17 H_2O to $ZnSO_4$, 800 H_2O. In a similar fashion it is calculable that to the value 1·105 should be added the amounts ·023, ·019, and ·008 volt when sulphuric acid of sp. gr. 1·090, 1·050, and 1·010 respectively surrounds the zinc, giving the sums 1·128, 1·124, and 1·113 respectively. Hence, finally, the following tables of values result:—

Zinc surrounded by Zinc-sulphate solution.

Electromotive force corresponding to net chemical action.	Electromotive force observed.	
$1{\cdot}105+x$, where x is a small quantity varying with the physical condition of the copper deposited.	Bright zinc opposed to bright copper	1·122
	Bright zinc opposed to electro-copper	1·116
	Bright zinc opposed to amalgamated copper . . .	1·115
	Amalgamated zinc opposed to bright copper	1·120
	Amalgamated zinc opposed to electro-copper	1·114
	Amalgamated zinc opposed to amalgamated copper . .	1·113
	Electro-zinc opposed to bright copper	1·121
	Electro-zinc opposed to electro-copper	1·115
	Electro-zinc opposed to amalgamated copper . . .	1·114

Zinc surrounded by dilute Sulphuric Acid.

Strength of acid.	Calculated electromotive force.	E.M.F. observed with electro-copper opposed to amalgamated zinc.
H_2SO_4, $358H_2O$=sp. gr. 1·010	$1·113+x+y$	1·129
H_2SO_4, $67H_2O$= „ 1·050	$1·124+x+y$	1·139
H_2SO_4, $37H_2O$= „ 1·090	$1·128+x+y$	1·148
H_2SO_4, $17H_2O$= „ 1·175	$1·141+x+y$	1·169

where y is a small quantity corresponding to the heat absorbed on dilution of the zinc sulphate to $ZnSO_4, 800H_2O$.

It is hence evident that in all cases the agreement between the E.M.F. actually developed and that due to the net chemical and physical actions taking place is so close, that what differences exist lie within the limits of experimental error; so that, finally, the conclusion may be drawn that, under favourable conditions, the E.M.F. of a Daniell cell is that due to the net resultant of the various physical and chemical actions taking place, the whole of the energy being adjuvant, viz. that gained by the displacement from copper-sulphate solution of copper by zinc, together with that gained by the transformation into ordinary electro-metal of the "nascent" copper first thrown down by the action; whilst under other conditions the E.M.F. falls below this amount, even after making allowance for the effect of the migration of the ions in causing solutions of different specific gravities to accumulate round the plates, indicating non-adjuvancy of one or other or both of these component portions of the total energy gained.

Experiments made with a view to find whether the Fall in E.M.F. on increasing the Current-density is mainly dependent on changes taking place in connexion with the actions at the surface of the Zinc or at that of the Copper plate.

115. In order to trace out somewhat more completely, if possible, how far that amount of fall in the E.M.F. of a Daniell cell taking place as the current generated increases, which is not due to the accumulation of solutions of zinc and copper of different densities round the plates, can be attributed to actions taking place at one or the other plate respectively,

the experiments described above (§103, 104) were repeated, with the difference that, instead of two sets of readings only being made (viz. when the two larger and the two smaller plates respectively were opposed), four sets were made—(1) when the two larger plates were opposed, (2) with the larger zinc and smaller copper, (3) with the two smaller plates, and (4) with the smaller zinc and larger copper plates opposed. By interpolation from the direct observational results, the differences of potential between the plates for any constant current-value were then calculated in each of the four cases. By comparing the values thus obtained in cases (1) and (2) and in (4) and (3), two sets of differences were obtained, indicating the effects produced by halving the area of the copper plate, every thing else being the same throughout, saving that with results (1) and (2) the larger zinc plate, and with the other pair of results the smaller zinc plate, was opposed to the two copper plates respectively: although this modified the actual values obtained in each of the original sets of readings, yet it produced practically no effect on the differences. In just the same way, by comparing the interpolation values in cases (1) and (4) and in (2) and (3), two corresponding sets of differences were obtained, indicating the effects produced by halving the area of the zinc plate; as before, the two sets substantially coincided. Various experiments of this kind were made with different plate-materials and fluids surrounding them: whilst the numerical values obtained were found to be to some extent variable with these conditions, yet, on the whole, it was always found that *the effect of halving the area of the copper plate notably exceeded that of halving the area of the zinc plate.* For instance, the following numbers were obtained in one set of observations with a cell containing bright pure zinc plates surrounded by zinc-sulphate solution of sp. gr. 1·42, and freshly-coated electro-copper plates surrounded by copper-sulphate solutions of sp. gr. 1·175, the larger plates exposing a surface of 5·0 square centimetres, and the smaller ones exposing 2·5 square centimetres.

	Effect of halving the area of the copper plate.						
Current, in micro-ampères.	Larger zinc plate opposed.			Smaller zinc plate opposed.			Mean difference.
	(1) Larger copper.	(2) Smaller copper.	Difference.	(4) Larger copper.	(3) Smaller copper.	Difference.	
100	1·082	1·078	·004	1·076	1·073	·003	·0035
200	1·071	1·066	·005	1·064	1·060	·004	·0045
500	1·061	1·055	·006	1·052	1·047	·005	·0055
1,000	1·045	1·036	·009	1·035	1·026	·009	·0090
2,000	1·026	1·015	·011	1·011	·999	·012	·0115
5,000	·964	·947	·017	·950	·929	·021	·0190
10,000	·876	·843	·033	·854	·820	·034	·0335
20,000	·729	·672	·057	·698	·639	·059	·0580

	Effect of halving the area of the zinc plate.						
Current, in micro-ampères.	Larger copper plate opposed.			Smaller copper plate opposed.			Mean difference.
	(1) Larger zinc.	(4) Smaller zinc.	Difference.	(2) Larger zinc.	(3) Smaller zinc.	Difference.	
100	1·082	1·076	·006	1·078	1·073	·005	·0055
200	1·071	1·064	·007	1·066	1·060	·006	·0065
500	1·061	1·052	·009	1·055	1·047	·008	·0085
1,000	1·045	1·035	·010	1·036	1·026	·010	·0100
2,000	1·026	1·011	·013	1·015	·999	·016	·0145
5,000	·964	·950	·014	·947	·929	·018	·0160
10,000	·876	·854	·022	·843	·820	·023	·0225
20,000	·729	·698	·031	·672	·639	·033	·0320

116. In precisely the same way, the following mean difference-values were obtained in two other analogous experiments, in the first of which the zinc plates were amalgamated and immersed in zinc-sulphate solution sp. gr. 1·42, the copper plates being also amalgamated and immersed in copper-sulphate solution sp. gr. 1·175; and in the second of which electro-copper plates and copper-sulphate solution sp. gr. 1·175 were employed, conjoined with amalgamated zinc plates immersed in dilute sulphuric acid sp. gr. 1·045. The above mean difference-values are also exhibited in the table.

Current, in micro-ampères.	Amalgamated zinc and amalgamated copper—zinc sulphate solution. Effect of halving area of		Amalgamated zinc and electro-copper—dilute sulphuric acid. Effect of halving area of		Bright zinc and electro-copper—zinc-sulphate solution. Effect of halving area of	
	Copper.	Zinc.	Copper.	Zinc.	Copper.	Zinc.
100	·001	·001	·001	·001	·0035	·0055
200	·004	·002	·002	·003	·0045	·0065
500	·009	·003	·003	·005	·0055	·0085
1,000	·015	·004	·005	·007	·0090	·0100
2,000	·024	·005	·011	·010	·0115	·0145
5,000	·040	·009	·020	·015	·0190	·0160
10,000	·049	·017	·041	·020	·0335	·0225
20,000	·058	·026	·056	·032	·0580	·0320

Much the same kind of result was obtained in various other similar experiments; the effect of halving the area of the copper plate was, especially with the stronger currents, much more marked than that of halving the area of the zinc plate. The actual value of the depreciation produced with the stronger currents in either case, moreover, clearly indicates that the diminution cannot psssibly be solely due to the formation of stronger zinc-sulphate solution round the zinc plate, and weaker copper-sulphate round the copper plate, in the case of the smaller areas, than are produced with the larger ones; and hence the conclusion is arrived at that when, by reason of the production of a current, the E.M.F. of a Daniell cell is diminished, and the energy developed becomes proportionately non-adjuvant, the non-adjuvancy is ascribable, not merely to actions taking place at the surface of the zinc plate, but also, and more particularly, to those taking place at the surface of the copper plate. In the former case, the energy gained during the conversion of the metallic zinc of the plate into solution of zinc sulphate makes its appearance under such conditions partly as heat from the very commencement of the action, and is never wholly manifested as electric action expressible in volt-coulombs. In the latter case, the effect of the chemical action of the cell is to set free copper, which, in its transformation from the condition in which it is first set free (nascent copper) to the condition ultimately assumed (more or less compact electro-

deposited soft coherent metal), causes a gain of energy which, like that due to the solution of the zinc, is partly manifested as heat *ab origine*, and is never, under such conditions, obtained wholly as electric action. The actual proportion of the energy due to the solution of the zinc or to the agglomeration (or allotropic modification) of the copper which is thus non-adjuvant, is variable within certain limits with the conditions of the experiment, the nature of the plate-surfaces and of the liquids in the cell, &c., but, *cæteris paribus*, is greater the stronger the current: with very feeble currents (of density not exceeding some 8 microampères per square centimetre), the proportion of non-adjuvant energy is too small to be measurable.

Summary of Results.

117. The above-described experiments, and the conclusions to be drawn from them, may be thus summarized:—

1. When a Daniell cell is constructed with equal-sized plates of pure zinc and pure copper (either compact bright metals, amalgamated plates, or plates covered with electro-deposited metal) immersed respectively in solutions of pure zinc and copper sulphates of the same specific gravity, and is made to generate a current not exceeding in density some 8 microampères per square centimetre, an E.M.F. is set up varying within certain small limits according to the precise condition of the surfaces of the metals as regards polish, oxidation, &c., but always lying fairly close to 1·115 volt, and practically identical with the E.M.F. corresponding to the energy gained in the net chemical change ensuing, viz. the displacement of copper from copper-sulphate solution by zinc; so that under these conditions practically all the energy gained is adjuvant, whether due to displacement of copper by zinc, or to transformation into ordinary metal of the "nascent" copper thus set free.

2. When impure zinc, or pure zinc amalgamated with impure mercury, is used, a greater or less amount of the energy gained is non-adjuvant, even under conditions such as would with pure zinc cause all the energy to be adjuvant. The source of this non-adjuvancy evidently lies in the nature of the actions taking place at the surface of the zinc plate;

the maximum E.M.F. that such a cell can generate is more or less considerably below 1·115 volt, in some instances by several per cent.

3. When the density of the current exceeds 8 micro-ampères per square centimetre, the E.M.F. of the cell falls more or less below 1·115 volt, owing to three causes, each of which produces an effect in the direction of diminishing the E.M.F. First, according as the current density is greater or smaller, a greater or lesser degree of non-adjuvancy of the energy gained in the conversion of metallic zinc into zinc-sulphate solution is brought about. Secondly, a greater or lesser degree of non-adjuvancy is similarly brought about in the energy gained by the transformation into ordinary copper of the "nascent" metal liberated at the surface of the copper plate; other things being equal, this source of non-adjuvancy distinctly predominates over the other source just mentioned. Thirdly, the passage of the current causes a weaker solution of copper sulphate to be formed round the copper plate, and a stronger one of zinc sulphate to be produced round the zinc plate, than were originally used; this sets up an inverse E.M.F., and diminishes the effective E.M.F. of the cell. The maximum possible diminution due to this cause does not exceed ·04 volt; whilst with a current the density of which amounts to ·003 ampère per square centimetre and upwards, the total diminution due to this cause together with the non-adjuvancy amounts to several times this maximum possible value. The diminution due to these various causes jointly constitutes what is sometimes spoken of as the "polarization" of the cell.

4. When the solutions of zinc and copper sulphate employed are not of the same specific gravity, the E.M.F. of the combination differs from that which would have been set up had both been of the same specific gravity by an amount which increases with the difference in specific gravity of the two solutions: if the copper-sulphate solution is the stronger, the E.M.F. is increased, and *vice versâ*. The amounts of increase and decrease are sensibly the same as the electromotive forces generated when two copper-sulphate or two zinc-sulphate solutions interdiffuse, the specific gravities of which are identical respectively with those of the two fluids actually present in

the cell examined. So long as the two solutions are of the same specific gravity, the E.M.F. set up is, *cæteris paribus*, sensibly independent of the actual value of this specific gravity; at least the fluctuations observed are not outside the range of experimental errors.

5. When dilute sulphuric acid is used instead of zinc-sulphate solution, its specific gravity being the same as that of the copper-sulphate solution, an increase in the E.M.F. of the cell is produced which sensibly corresponds with the increase in the "heat of formation" of zinc sulphate when sulphuric acid is employed of the strength used in the cell, as compared with acid diluted to a larger extent (H_2SO_4, 800 H_2O). If the copper-sulphate solution differs from the acid in specific gravity, the latter not exceeding 1·18 in specific gravity, the E.M.F. is raised above or lowered below what it would have been had the copper sulphate been of the same specific gravity, by an amount which is sensibly the same as the E.M.F. generated by the interdiffusion of two copper-sulphate solutions the specific gravities of which are identical respectively with those of the two fluids actually present in the cell examined.

6. Owing to the diminution in the E.M.F. of a Daniell cell when generating a current, the fluctuations in the maximum values obtainable with any given cell with the physical condition of the surfaces of the plates and the time that has elapsed since its construction, the tendency to deposition of copper on the zinc by diffusion and the consequent diminution in E.M.F., and the variation in E.M.F. according as dilute acid of different strengths or zinc-sulphate solution is used to surround the zinc plate, it results that "the E.M.F. of a Daniell cell" is a very variable standard of E.M.F. and one singularly devoid of permanence. By taking suitable precautions in the construction of a cell (using pure zinc amalgamated with pure mercury, amalgamated or electro-copper, and pure zinc- and copper-sulphate solutions of the same specific gravity), a cell may be obtained the E.M.F. of which does not differ more than $\pm 0·25$ per cent. from 1·113 or 1·114 volt, according as amalgamated or electro-copper is used; but such a cell cannot be kept many hours without altering in value materially, and is in practice a very far less

convenient standard than the mecurous-sulphate cell of Latimer Clark; for, notwithstanding that the limits of variation between two cells of this latter kind, similarly prepared, are somewhat greater than those of the best form of standard Daniell cell, it nevertheless possesses the valuable property of remaining sensibly constant (the temperature being the same) for many months, and even one or more years, as will be more completely shown in a future paper.

VIII. *On the Electric Resistance of Carbon under Pressure.* *By* Professor SILVANUS P. THOMPSON, *B.A.*, *D.Sc.**

§ 1. IT has often been stated that the electric resistance of carbon decreases when subjected (1) to an increase of temperature, (2) to a mechanical compression. The first of these statements has been verified by so many authorities that there can hardly be any question of its correctness. The second I believe to be wholly misleading; for some careful experiments that I have lately made lead to the conclusion that the effect of mechanical compression upon the electric resistance of dense carbon is almost, if not quite, *nil*, and that what has been mistaken for an increase in electric conductivity is in reality merely better contact at the points of junction with the circuit.

§ 2. A preliminary experiment to test whether the alleged decrease of resistance by pressure was due to a true increase in specific conductivity or to better end-contact, was made in the following manner:—A thin cylindrical rod of Carré's dense artificial carbon (such as is used in electric lamps) was taken, its length being 72 centimetres. At a point about one fourth of its length from the end a groove was filed round it, and around it was bound tightly the end of a clean thin copper wire. It was placed vertically upon a piece of copper in an upright frame, so that pressure could be applied longitudinally at the top; a flat piece of copper was placed upon the top of it and pressed lightly upon it. The point at which the thin wire was bound round it was then about 18 centim. from the lower end. The upper and lower portions were then

* Read February 25, 1882.

connected up with a Wheatstone's bridge, provided, as in Kirchhoff's pattern, with a wire of German silver stretched over a divided scale, the resistances of the two parts of the wire right and left being (when balance was obtained in the galvanometer) proportional respectively to the resistances of the two portions of the carbon rod. The relative, not the absolute, resistances of the two portions were therefore being measured. Two bichromate cells supplied the requisite current, the galvanometer being a short-coil astatic instrument of simple form.

On trying the resistances, it appeared that the resistance of the longer part, which was uppermost, was somewhat greater in respect to that of the lower and shorter part than would have been expected from their relative lengths; the ratio of their resistances being 81 : 19, or about 4·25 : 1, whereas their lengths were as 3 : 1 almost exactly. After taking the rod out of its place and putting it back again under light contact at the top as before, the ratio was found to be 82 : 18, or 4·55 : 1. The rod was thus removed and replaced several times; and the ratio of the resistances was found to differ somewhat every time, the figures varying from 4·7 : 1 to 3·92 : 1. Pressure was now applied at the top of the rod, and the ratio of the resistances was again measured. With a load of 5 kilogrammes (as much as it was judged the rod would bear without risking breaking it), the ratio of the resistances was found to be much more constant and much nearer to the ratio of the lengths of the two portions, being 75·3 : 24·7, which is not very different from 3 : 1. It was therefore clear that the previous values had been greatly affected by the differences in contact at the two ends; the lower contact having less resistance than the upper, in consequence of the superincumbent weight of the rod and connexions—about 19 grammes in total.

§ 3. Another rod of Carré's carbon was next examined, and its actual resistance measured in ohms. Its length was 42·6 centim., its diameter 0·48 centim. To prepare it for the experiment, it was electroplated with a thin coating of copper to the length of about 1 centim. at each end, the extremities being afterwards scraped bare of copper so that end-contacts should be made against the carbon itself. Copper wires were then carefully soldered to the copper coating at about 0·5

centim. from the ends. The object of this arrangement was to render possible a comparison between the resistance of the rod when there was merely end-contact—which might be more or less perfect according to the pressure—and the resistance of the rod as measured when there was a perfect contact through the deposited coatings of copper. The rod was then laid in a horizontal frame, where it reposed on two Y-shaped bearings—one end pressing against a lever-arrangement for the purpose of putting on a measurable amount of pressure, the rod being fixed at the other end by abutting against a brass set-screw. A copper piece was introduced between the lever and the extremity of the rod, in order to provide an end-contact; and arrangements were made whereby the rod could be connected up in a Wheatstone-bridge, the connexion being made at pleasure either through the end-contacts or through the copper-plated junctions.

The resistance of the rod between the copper-plated junctions was then measured, the rod being free at both ends. It was found to be $0{\cdot}56 \pm 0{\cdot}007$ ohm. The end-contacts were then made to touch lightly (the circuit through them remaining open). The resistance through the copper-plated junctions showed no change. Pressure was then applied to the rod longitudinally, and augmented until it began to show lateral distortion, the effective force along the rod being 4150 grms., equivalent (if the area of cross section of the rod be taken as 0·18 square centim.) to 23,055 grms. per square centimetre. Yet, even under this pressure, not the smallest change could be detected in the resistance between the copper-plated junctions. If there was any, it was certainly less than 0·005 ohm, or less than 1 per cent. of the whole resistance.

The circuit was now made through the end-contacts by moving the set-screw until the lightest possible contact was obtained, the connexions through the copper-plated junctions having been thrown out of circuit. The resistance thus determined was 1·1 ohm. Pressure was applied as before. The resistance fell to 0·72 ohm when the pressure of 23 kilogrammes per square centimetre was reached. On releasing the pressure, the resistance again rose until contact became as light as possible. The resistance attained 1·08 ohm, when it rose abruptly to infinity as the set-screw ceased to touch the

end of the rod. The battery and galvanometer used throughout were the same as described above.

Nothing could be more significant than these observations. When perfect contact was ensured by electroplating, pressure produced no effect on the resistance of the carbon rod, or one inappreciably small. When circuit was made by pressing pieces of copper and brass against the rough ends of the carbon rod, contact was only imperfectly obtained, and the resistance varied with the pressure because increased pressure brought about better contact, or contact at a greater number of points.

The bearing of these observations upon the theory of the carbon rheostat, the carbon relay, the carbon transmitting-telephone, and the carbon microphone is obvious.

University College, Bristol,
February 1882.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

JULY 1882.

IX. Regnault's *Determination of the Specific Heat of Steam.* *By* J. MACFARLANE GRAY*.

REGNAULT'S experiments on the specific heat of vapours have been interpreted by Regnault as giving results not at all in accordance with the deduction from the kinetic theory of gases, that, for matter travelling in single molecules, the product of the molecular weight by the specific heat is a constant for all substances. I have been led, by considering the order of temperature-pressures for steam, to conclude that the above deduction is true for steam; and I have no doubt, also for all matter travelling in single molecules. When, in 1880, I laid my conclusions before the Physical Society as being corroborated by Regnault's dynamical experiments, it was objected that Regnault's direct thermal experiments gave results widely different from my conclusions; and the report on my paper was that that difference proved that, in the corroborations I had pointed out, I had been led away by merely numerical coincidence.

I will now show that Regnault's thermal experiments have been misinterpreted by Regnault himself, and that he ought to have read the specific heat of steam, according to his experi-

* Read February 25, 1882.

ments, to be exactly in accordance with the deduction of the kinetic theory.

The method of the experiments was to generate steam at 100° C., to superheat it under atmospheric pressure to (say) 125° in one set of experiments, condensing it in a calorimeter to ascertain what quantity of heat was given up, down to 0°. In a second set of experiments with the same apparatus, the temperature was raised to (say) 225°, while the steam was still at atmospheric pressure; this was also condensed in the same calorimeter. It was found that the heat given up by the steam at 225° exceeded that given up by the steam at 125° by 48·051 units of heat; and, dividing by the difference of temperature (100), Regnault found 0·48051 for the specific heat of steam at constant pressure.

In these experiments the superheating to 125° was, no doubt, intended to thoroughly dry the steam, so as to get, in the quantity of heat abandoned in the first set of experiments, the entire heat of complete gasefication, because, if still some moisture remained in the steam, the latent heat of evaporation of that moisture would be included in the difference-quantity supposed to be due to the specific heat of temperature-raising, and, by that amount, the result would be too high.

It appears to me that the completion of the evaporation of suspended moisture cannot be accomplished between 100° and 125°; but it will be more likely to be carried on between 125° and 225°. Particles of liquid remain at the pressure-temperature whatever be the superheated temperature of the gas in which they are suspended. The rate of evaporation of those particles will therefore depend upon the temperature-difference; and in the lower range of temperatures but little of the moisture will be evaporated in its rapid passage through a worm heated to only 25° in excess of the pressure-temperature. In the second set of experiments the excess of temperature was 125°.

To test this, let similar experiments be made at 100°; and if the resulting apparent specific heats between 100° and 125° are higher than those obtained by Regnault between 125° and 225°, then my argument is demolished; but if the results are smaller, then my suspicions have been justified, and a correction is required on the results as given by

Regnault. Fortunately for my object, Regnault has left us (in vol. i., at page 695) the data of thirty-eight reliable experiments on the "total heat" of steam at 100° down to 0°, giving the mean = 636·70 units of heat. I have, with this "total heat" for 100°, compared the results of the experiments on the steam of 125°—in precisely the same way as Regnault worked for the interval between 125° and 225° (these experiments are recorded in vol. ii., pages 167–178). The results of my calculations are given below. The first two series were merely preparatory trials to arrive at the best form of apparatus; and only a few grammes of vapour were passed through the worm at each of those experiments. In the third and the fourth series ten times as much vapour was passed through on each occasion. Regnault says:—"J'ai pu opérer ainsi sur des quantités de vapeur beaucoup plus considérables, et diminuer l'importance relative des corrections produites par les causes perturbatrices." This consideration is still more important when the range of temperature is limited to 25°—only one fourth of the range from which Regnault deduced his result. The large calorimeter was used only in the third and fourth series.

First series.	Second series.	Third series.	Fourth series.
·280	·546	·299	·386
·465	·591	·314	·381
·417	·567	·340	·346
·377		·375	·309
·463	Mean ·567	·400	·427
		·411	·463
Mean ·400		·349	
		·413	Mean ·3853
Preparatory trials.		·405	
		·415	
		Mean ·3721	

That the method of calculation may be perfectly clear, I give the particulars for the first experiment in the fourth series. Superheating to 124°·81, each unit of vapour gave up 646·28 units of heat down to 0°. Steam at 100° gave up 636·70 down to 0°.

$$\frac{646{\cdot}28-636{\cdot}70}{124{\cdot}81-100}=\cdot386.$$

The quantity of vapour in this experiment was 102·62 grms.; the quantity in the first of the first series was only 8·957 grms.

The fourth series was made with an apparatus which was an improvement on that used in the third series; and, taking that series only, adding the probable amount of moisture which would remain at 100° temperature (say 1 per cent. on the ·385), the result is ·389; this, on other grounds, I believe to be nearly correct. If, however, neglecting the other grounds for my opinion, we take the mean of the means of the third and the fourth series, we get

$$\frac{\cdot 3721 + \cdot 3853}{2} = \cdot 3787.$$

This is what Regnault might fairly have done.

If we now calculate what Regnault's experiments would give as the kinetic-theory result, we get the specific heat of hydrogen, at page 121, "mean = 3·4090;" and taking the molecular weight of steam $= 17 \cdot 96$, we find the specific heat of steam

$$3 \cdot 409 \times \frac{2}{17 \cdot 96} = \cdot 3796.$$

That is to say, the kinetic theory and the thermal determination give almost identically the same number.

This is, I believe, the first experimental proof that the law of "inversely as the molecular weights" applies to compound gases.

X. *The Effect of Temperature on the Electrical Resistance of Mixtures of Sulphur and Carbon.* *By* SHELFORD BIDWELL, *M.A., LL.B.**

SINCE December 1880 I have from time to time made a number of experiments with the object of ascertaining whether sulphur could, under any circumstances, be made to exhibit the remarkable property possessed by selenium, and in a smaller degree by tellurium, of having its electrical resistance diminished by the action of light.

Amongst other things, the effect was tried of mixing it with

* Read March 25, 1882.

graphite. The sulphur was heated to a temperature a little above its melting-point (115° C.), and when quite liquid a small quantity of tolerably pure powdered plumbago was stirred into it. The liquid mixture was then poured into moulds and allowed to cool quickly. Sticks or plates were thus produced of a substance which in general appearance closely resembled crystalline selenium, its surface being dull and slate-coloured, and its fracture metallic, not unlike that of cast iron.

A short stick of this substance was fitted with platinum electrodes (platinum wires being made red hot and pressed into the two ends), and was joined up in circuit with a Leclanché cell and a reflecting galvanometer. The spot of light was brought to the zero-point at the middle of the scale by means of a magnet, and the sulphur rod exposed to the radiation of a gas-flame which was held within a few inches of it. The spot at once began to move, showing a considerable change in the resistance of the rod. The gas-flame was extinguished; and the spot slowly returned to zero. Upon examination, however, it appeared that the behaviour of this sulphur rod differed from that of crystalline selenium in two important particulars. In the first place, the change of resistance was clearly an effect not of light, but of temperature. Burning magnesium produced no greater change than the gas-flame when held at the same distance. Sunlight was found to have a powerful effect, which was scarcely diminished when the light was caused to pass through red glass; but blue glass or a cell containing water formed an effectual screen. A black hot poker, or even the warmth of the finger, caused a greater deflection than a strong light which was too far removed to heat the rod sensibly.

Now there can be little doubt that the decrease in the resistance of selenium which occurs under the influence of radiation is totally distinct from any effect produced by temperature. It is of course true that absorption of radiations is followed by a rise of temperature, particularly when such radiations belong to the red and infra-red part of the spectrum ; but it is not, I think, the fact that such rise of temperature is in any way connected with the remarkable variation of resistance under the influence of light, which, owing to

the invention of the photophone, has lately attracted so much attention. The electrical effects of radiation are, in this case at least, no more due to rise of temperature than are its chemical effects. The evidence in favour of this assertion seems to me to be overwhelming; and I hope to return to it upon another occasion, with special reference to the paper in support of the opposite view read last year by Dr. Moser.

The second point in which the sulphur rod appeared to differ from selenium, was in the *direction* of the change which the radiation of the gas-flame produced, the galvanometer showing that its resistance was increased, instead of being diminished as might have been expected. Here, however, upon the supposition that the effect is due to heating, the behaviour of the sulphur is really similar to that of selenium; for my experiments (agreeing with those of Prof. Adams) show that at ordinary temperatures slight heating is always accompanied by considerably increased resistance.

After numerous trials, in order to ascertain what proportions of sulphur and graphite yielded the greatest sensitiveness to heat, it appeared that a mixture containing 20 parts by weight of sulphur to 9 of graphite was the most suitable. It was also found to be more sensitive when cooled rather quickly than when cooled slowly, though its specific resistance (which is always high) was generally lower in the latter case.

In order that the peculiar property of the substance may be exhibited in the most effective manner, it is necessary to arrange it in thin films, so that a large surface relatively to the bulk may be exposed to the action of radiation. This was at first done by spreading it as thinly and evenly as possible upon plates of mica having tinfoil electrodes at each end. An objection to this method, however, was the enormously high resistance which it involved, amounting sometimes to several hundred thousand ohms. I therefore adopted the device which I generally use in the construction of selenium cells, and which is a simple modification of a plan originally proposed by Dr. Werner Siemens.

Two wires, preferably of platinum, are wound parallel to each other, and very close together, around a slip of mica, care being taken that the wires do not touch each other at

any point. A film of the melted mixture is spread evenly over one surface of the mica; and the wire electrodes are thus connected with each other through half their entire length by a series of very narrow strips of the sulphur mixture.

The resistance of a sulphur "cell" constructed in this manner was 9100 ohms at a temperature of 14° C. The cell was slowly heated in an air-bath, and the resistance measured as accurately as possible at almost every degree. The following table sufficiently indicates the rate at which the resistance increased.

Temperature.	Resistance.
14° C.	9,100 ohms.
19°	11,400 "
25°	13,700 "
30°	16,700 "
35°	20,600 "
40°	26,900 "
45°	34,000 "
50°	42,900 "
55°	57,000 "

Thus at 55° the resistance of the cell was more than six times as great as at 14°.

Upon another occasion the cell was raised to a much higher temperature. No measurements were made with the bridge, however, the cell being simply connected with a battery and a galvanometer, and the deflections noted from time to time. After passing 55°, the resistance increased with great rapidity, until at a temperature of 100° it was sensibly infinite, there being no movement of the spot of light when the circuit was opened with a key. Immediately after passing 100°, the resistance began to fall even more quickly than it had risen. The deflection at 105° was the same as that at 85°; at 110° the same as at 65°; at 114° the same as at 50°; and at 115° the same as at 35°. The spot of light now became so unsteady (probably in consequence of the melting of the cell) that it was not possible to carry the experiment further.

A mixture has also been prepared in which lampblack was used instead of graphite; but very few experiments have yet been made with it, and the results obtained have not been

uniform. At ordinary temperatures it generally behaves like the graphite mixtures; but its temperature of maximum resistance is probably lower than 100°. In one case indeed, in which the proportions were 8 parts of sulphur to 1 of lampblack, the resistance was found to be greatest at 15° (530,000 ohms), any change in the temperature, whether in the direction of heat or cold, producing a decrease. But since no such effect has been produced with other specimens of the same compound, I am inclined to think that it is due to some unnoticed peculiarity in the construction of the cell.

With this single exception, every specimen of the mixtures, whether made with graphite or lampblack, has at ordinary temperatures been found to have its resistance increased by heat.

At first sight this appears to be a very paradoxical phenomenon. It is now generally admitted that the resistance of graphite and other forms of carbon is diminished by heat; and it is also commonly believed that a rise of temperature invariably causes a decrease in the resistance of insulators such as sulphur. The compound of sulphur and carbon with which we have to do is certainly only a mechanical mixture (for no chemical combination could be formed at a temperature of 150° C., which is never exceeded in making the preparation); yet the effect which heat produced upon it is exactly opposite to that produced upon each of its constituents.

Some experiments were made in order to determine the effect of heat upon the resistance of carbon and sulphur separately; but, although in the case of sulphur some unexpected results were obtained, nothing whatever was observed which would *per se* account for the variations of resistance in the sulphur and carbon mixtures.

I believe the true explanation to be this:—The mixture does not consist of a uniform structureless mass of sulphur having particles of carbon imbedded in and completely surrounded by it. It is in fact an aggregation of little crystals of sulphur, with carbon packed between them like mortar between bricks. The conduction thus takes place entirely through the carbon particles, which may be considered as extending in a series of chains from end to end of the mass. Under the influence of

heat, both the sulphur and the carbon expand; but the expansion of the sulphur is nearly ten times as great as that of the carbon, the net result being that the carbon particles are drawn apart, and have fewer points of contact with each other. The number of complete chains is thus diminished, and the resistance of the mass consequently increased.

It is stated in Balfour Stewart's 'Treatise on Heat,' upon the authority of Kopp, that sulphur, after being heated to a certain temperature, contracts instead of expanding. This would account for the fact that, after attaining a certain temperature, the resistance of the mixture begins once more to decrease; for the carbon particles would, when the sulphur contracts, be brought together again.

Several experiments corroborate this explanation of the variation of the resistance. Thus a mixture was made containing 3 parts of shell-lac to 2 of graphite. Though the proportion of carbon was larger than in the sulphur experiments, its resistance was found, as was expected, to be infinite; for the structureless shell-lac penetrated between and completely surrounded the carbon particles. A mixture of paraffin and graphite gave the same result.

A short rod of the usual sulphur-and-graphite compound (20 parts to 9) was fitted with platinum-wire electrodes; and its resistance in the air at 17° was found to be 3170 ohms. The rod was immersed in a beaker of turpentine at the same temperature, and its resistance almost immediately went up about 800 ohms; and in five minutes it was somewhat higher. Though this result did not at first appear to be easily intelligible, it is fully explained by supposing that the liquid penetrated between the sulphur crystals and surrounded the carbon particles. In order to increase the effect, the beaker of turpentine containing the rod was placed under the receiver of an air-pump and the air exhausted. After remaining for a quarter of an hour in a vacuum, air was admitted, and another quarter of an hour was allowed for the action of the atmospheric pressure. At the expiration of this time its resistance was found to have increased to 15,600 ohms, about five times as great as it was originally. The rod was then placed upon blotting-paper; and three days afterwards its resistance was

2970 ohms, the temperature of the air being 16°. This was 200 ohms lower than when it was first made; but the temperature was one degree lower.

Thinking that the oil of turpentine might possibly have had some action on the sulphur, I repeated the experiment with olive-oil. On first immersion, the resistance went up from 2970 to 3150 ohms, and in six minutes to 3770 ohms. It was then left in a vacuum for forty-five minutes, after which the air was admitted, though the effervescence had not even then quite ceased; and when measured, after sufficient interval to allow the oil to penetrate, its resistance had increased to 8140 ohms. The comparative smallness of the effect produced in this latter experiment was probably due to the greater viscidity of the oil.

Lastly, since the behaviour of the sulphur-and-graphite mixture under the influence of heat seemed, like that of the microphone, to depend upon the interaction of contiguous particles of carbon, it was expected that one of the sulphur-cells before described might be used as a telephone-transmitter. This, upon trial, was found to be the case. With a single Leclanché the sounds reproduced in the receiving-telephone were feeble; but well-known nursery-rhymes were easily recognized. When, however, twenty Leclanchés were used, the sounds were much louder and the articulation perfectly distinct. No experiments whatever have yet been made with the view of developing this property; and there can be little doubt that it is capable of extension. It is not impossible that a transmitter constructed upon this principle might be found to be of practical value; and it is entirely different, at least in appearance, from any thing which has been made before.

The sulphur-cell might also perhaps be used as a thermoscope, being simpler and more easy to construct than an ordinary thermopile.

It has recently been proposed to use mixtures of sulphur and graphite for making cheap resistance-boxes. It is needless to point out that the great sensitiveness of such compounds to small changes of temperature renders them very unsuitable for such a purpose.

DIAGRAM No. 1.

May 6th 1881.

Experiment on 3" Lead Balls melted in Fluid Lead.

Note.– *The lead was very hot during Expt. 43. and gradually cooled during Expts. 44 & 45.*

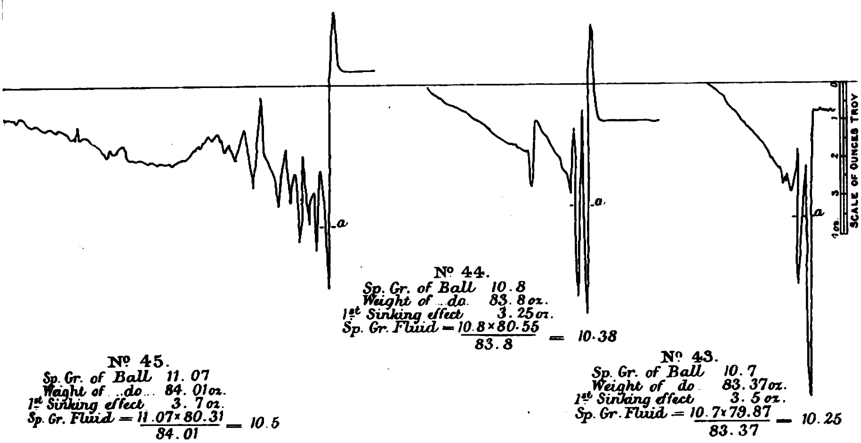


DIAGRAM No. 2.

May 18th 1881.

Experiment on 3" Tin Ball melted in Fluid Tin.

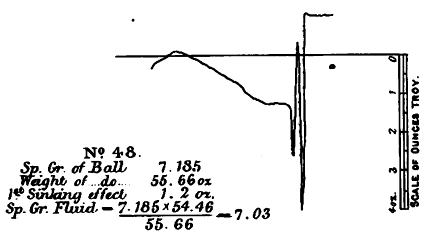
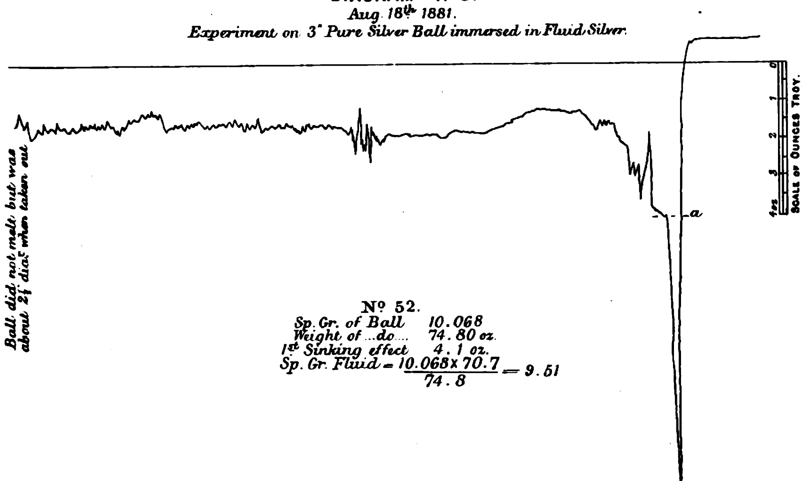


DIAGRAM No. 3.

Aug. 18th 1881.

Experiment on 3" Pure Silver Ball immersed in Fluid Silver.



Mintern Bros. lith.

XI. *On the Fluid Density of certain Metals.* By W. Chandler Roberts, *F.R.S., and* T. Wrightson, *Memb. Inst. C.E.**

[Plate V.]

At a Meeting of the Physical Society, on February 12, 1881†, we communicated the results of certain experiments, made with the aid of the oncosimeter, on the fluid density of bismuth, in which we showed that the densities in the fluid and cold solid states are respectively 10·055 and 9·82, the fluid density thus being 2·4 per cent. higher than the cold solid. These determinations correspond closely with results that had been obtained by one of us, using the "cone" method devised by Mr. Mallet‡. For the sake of convenience we here reproduce our results on bismuth.

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Floating effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
31.	2	23·33	9·72	1·0	10·13	Bismuth ball in fluid bismuth.
32.	2·25	22·184	6·99 (iron)	9·3	9·92	Iron ball in fluid bismuth.
33.	do.	33·46	9·755	1·3	10·11	Bismuth ball in fluid bismuth.
34.	do.	33·37	9·757	·6	9·94	do.
35.	do.	33·53	9·774	·7	9·98	do.
36.	do.	22·184	7·02 (iron)	10·2	10·25	Iron ball in fluid bismuth.
				Mean	10·055	
		Specific gravity of solid bismuth			9·82	

Since these results were published we have extended the

* Read January 28, 1882.

† Proc. Phys. Soc. vol. iv. p. 195; and Phil. Mag. [5] vol. xi. 1881, p. 295.

‡ Proc. Roy. Soc. vol. xxii. p. 366, and vol. xxiii. p. 209.

experiments to the following metals—copper, lead, tin, zinc, silver. The results are now submitted to the Society.

Copper.

Six experiments were made with the oncosimeter* on copper; four of these were made with cast copper balls 2¼″ diameter, one with a cast-iron turned ball 2¼″ in diameter, and one with a copper ball 3″ in diameter. The first four were made with a spring that lengthened ¼ inch per ounce troy; the last two with a more sensitive spring, giving ½ inch elongation per ounce troy. It will be seen by the accompanying table that the fluid density does not vary much, although, as has just been stated, the conditions were varied.

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
37.	2·25	26·357	8·23	nil.	8·23	Copper ball in fluid copper.
38.	do.	27·005	8·463	·2	8·4	do.
39.	do.	26·25	8·288	·5	8·13	do.
40.	do.	25·923	8·281	·2	8·217	do.
41.	do.	22·186	7·02 (iron)	Floating effect 3·5	8·127	Iron ball in fluid copper.
42.	3	64·237	8·61	Sinking effect 3·0	8·2	Copper ball in fluid copper.
				Mean	8·217	
			Specific gravity of solid copper	....	8·8	

The mean of these results gives 8·217 as the density of fluid copper. This metal melts at a temperature which exceeds 1000° C., and, when melted in air, absorbs oxygen. We found, for this reason, that it was not possible to obtain trustworthy results by Mallet's method, even though the cone was filled with an atmosphere of coal-gas.

* A description of this instrument was published in the Journal of the Iron and Steel Institute, part 2, 1879, p. 418.

Lead.

Three experiments were made with the oncosimeter, the results being as follows:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
43.	3	83·37	10·7	3·5	10·25	Lead ball in fluid lead.
44.	3	83·8	10·8	3·25	10·38	do.
45.	3	84·01	11·07	3·7	10·5	do. Fluid lead getting cold.
				Mean	10·37	
			Specific gravity of solid lead	....	11·4	

A mean of two experiments on lead, made on Mallet's system, gave 10·65 as the liquid density, the results being 10·63 and 10·66 respectively.

The diagram No. 1 (Plate V.), gives the results of the experiments on lead; and the calculations of fluid density are in each case attached, the point *a* indicating in each the position taken as giving the true initial sinking effect at the moment of immersion.

Tin.

Two experiments were made with the oncosimeter, with the following results:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
46.	3	55·66	7·165	1·1	7·02	Tin ball in fluid tin.
47.	Failure.					
48.	3	55·66	7·185	1·2	7·03	do.
				Mean	7·025	
			Specific gravity of solid tin	....	7·5	

The diagram No. 2 is reduced from the oncosimeter diagram, experiment 48.

A mean of three experiments on tin, made on Mallet's method, gave 6·974 as the liquid density—the results being 6·903, 6·982, and 6·976 respectively.

Zinc.

Three experiments were made with the oncosimeter on this metal, the results being tabulated below:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
49.	3	52·52	6·893	2·4	6·57	Zinc ball in fluid zinc.
50.	3	52·58	6·901	3·3	6·47	do.
51.	3	52·32	6·903	3·75	6·4	do.
				Mean	6·48	

Specific gravity of solid zinc 6·8 to 7·2 (Rankine).

The crucible was kept in the furnace during the three experiments, and maintained as nearly as possible at the same temperature throughout.

Only one determination of this metal has been made by Mallet's method, giving a liquid density of 6·55.

Silver.

We looked with much interest to the experiments on the density of molten silver for several reasons—mainly from the fact that it melts at a high temperature, which has been determined with great care by the late Henri Ste Claire Deville, who in his later experiments on the subject fixed the temperature at 940° C.* We operated on a thousand ounces of silver, which proved on assay to contain 998 parts of pure silver in 1000. We have as yet only secured one trustworthy result, as we found it very difficult so to adjust the temperature of the molten silver as to prevent the silver balls from melting either too rapidly or too slowly. In the experiment No. 52, in which we did secure a result, the metal must have been just about its melting-point, as at the end of four

* *Comptes Rendus*, t. xc. (1880) p. 773.

minutes the silver ball had not entirely melted; the experiment was stopped, however, as the silver began to solidify on the iron stem of the ball.

The diagram No. 3 is reduced from the original; and we have appended the calculation for fluid density, assuming the position of equilibrium at first immersion of the ball to be at point *a*.

The particulars of the experiment are as follows:—

No. of expt.	Diameter of ball, in inches.	Weight, in troy ounces, including the stem for attachment.	Specific gravity of cold ball, including the stem.	Sinking effect on first immersion, in troy ounces.	Deduced specific gravity of fluid metal.	Remarks.
52.	3	74·8	10·068	4·1	9·51	Pure silver ball in pure silver.

The liquid density of pure silver, as determined by one of us*, on Mallet's method, from a mean of two experiments, giving respectively 9·447 and 9·476, was 9·46. This confirms the single result obtained by the oncosimeter.

If we now tabulate the mean results of the foregoing experiments, as well as those previously obtained, we shall be able to compare the densities according to the two methods of investigation:—

Fluid density of	By Mallet's method.	By oncosimeter.
Bismuth	10·039	10·055
Copper	......	8·217
Lead	10·65	10·37
Tin.................	6·974	7·025
Zinc	6·55	6·48
Silver..............	9·46	9·51

The difficulties connected with the working of the oncosimeter are less than those which arise in Mallet's method; and the sources of error are less, the correctness of the result in the latter depending on the metal in the cone being free from air-holes.

It will be seen from the above table that, in the case of

* Roberts, Proc. Roy. Soc. vol. xxiii. p. 493.

the five metals in which both systems were tried, the results correspond as closely as could be expected, taking into consideration the great difficulties of observation at the high temperatures employed. We therefore venture to think that the oncosimeter can be depended upon generally for the determination of the fluid densities of metals, if the experiments are conducted with the necessary care.

Taking therefore the oncosimeter-results as approximately accurate, we find the change of volume of these metals in passing from the cold solid to the liquid state to be as follows :—

Metal.	Specific gravity of solid.	Specific gravity of liquid.	Percentage of change in volume from cold solid to liquid.
Bismuth	9·82	10·055	Decrease of vol. 2·3
Copper	8·8	8·217	Increase of vol. 7·1
Lead	11·4	10·37	do. do. 9·93
Tin	7·5	7·025	do. do. 6·76
Zinc	7·2	6·48	do. do. 11·1
Silver	10·57	9·51	do. do. 11·2
*Iron (No. 4, foundry, Cleveland)	6·95	6·88	do. do. 1·02

The experiments just described lead us to believe that, although the conditions under which they are made may render it impossible to obtain results that are rigidly accurate, still the errors are inconsiderable. It has been urged that the unsoundness or porosity in the casting would disturb the accuracy of the results; but this is provided against in each case by taking the specific gravity of the ball operated on, its iron stem being submerged in water to a point which is afterwards just reached by the molten metal. It has also been suggested that the expansion of the ball, when it enters the fluid metal, causes tension on the metal in the interior of the ball, and that therefore the density of the ball (from which the fluid density is determined) can never be trusted. It must be remembered, however, that the determination of the fluid density is made and automatically registered at the moment of immersion, before the volume of the ball can be practically affected.

* Wrightson, Journal of the Iron and Steel Institute, No. 1 (1880), p. 20.

Since our last paper was read, we have seen that MM. F. Nies and A. Winkelmann* have been investigating the changes in volume of certain metals when melted. Their method will be best understood by a short description of the way in which the metal tin was treated by them.

Tin was melted in a suitable vessel, the melting-point (viz. 226°·5 C.) being carefully maintained. Pieces of tin were then dropped in, and observed first to sink, then to rise before melting—showing that solid tin has a higher specific gravity than the liquid mass, but that when its temperature has been raised to the melting-point, or rather to a certain temperature just below, then it is specifically lighter. In order to determine this relation, they enclosed pieces of copper (having a higher specific gravity than tin) in pieces of tin, and were thus able to increase the specific gravity of the solid piece at will; and although not able to determine directly whether the compound piece was of the same density as the liquid, the latter not being transparent, yet by varying the amount of copper enclosed in the tin, thus forming pieces of a different density, they were enabled to say that it lay between the two limiting values. Compound pieces with varying and known amounts of copper, thus formed, were placed on the surface of the liquid tin for some little time, then dipped under the surface; and it was noted whether they rose again or not. The pieces were each then carefully taken out again and weighed, to ascertain that no part had been melted. This was continued until they found two compound pieces such that, at a temperature just below the melting-point, one would just rise and the other would just sink.

If, then, the relative densities of the liquid tin and the copper at the temperature of liquid tin could be ascertained by experiment, the relative density of the solid tin just below melting-point and the liquid tin can be calculated.

A copper ball was heated to the temperature of the melting tin, and weighed, first in air, then in the liquid tin. The relative densities being thus ascertained, the calculations, which we need not give here, are of a simpler character.

In the case of tin, they found that the liquid is of ·7 per cent.

* *Sitzungsberichte der Akademie der Wissenschaften zu München*, 1881, part 1, p. 63.

greater density than the hot solid at a temperature just below melting-point.

This, so far as it goes, is confirmatory of the results obtained by us. If diagram No 2. be examined, it will be seen that the line of volume rises gradually to the line of equilibrium, indicating expansion; and just before the ball melts rises above the line, indicating a floating effect, when the temperature approximates to the melting-point.

Tin therefore appears to be similar to iron; viz. it is at its maximum density when cold, and at its minimum density when at a temperature just below melting, and that the fluid-density is between the two.

The results obtained by MM. Nies and Winkelmann on bismuth will serve to illustrate the degree of approximation of these limiting values.

A piece of bismuth, when allied with a platinum block weighing 21·76 grammes, sank; and another piece, weighing 35·4 grammes, floated; which is equivalent to stating that the specific gravity of the melting bismuth is between 10·28 and 10·12.

They arrive at the general result that not one of the eight metals they examined will justify the assertion that "bodies contract on becoming solid;" but the experiments rather favour the view that metals when solid, *at a temperature close upon their melting-points*, are less dense than when molten.

Without accepting MM. Nies and Winkelmann's results as final, we do not consider them to be opposed to our own, as theirs relate solely to the ratio of the densities of the solid and liquid metals at as nearly as possible the same temperature, while our experiments were undertaken with a view to determine the actual density of a metal at the *lowest* temperature at which it is perfectly liquid.

XII. *Experiments on the Faure Accumulator.* *By* Professors W. E. Ayrton *and* John Perry*.

Having made, at the request of the Faure Accumulator Company, a series of experiments on some of their cells, we have thought that a short account of some of the results obtained

* Read February 25, 1882.

may not be uninteresting to the members of the Physical Society.

The object of the experiments was to ascertain, *first*, the efficiency of a cell—that is, the ratio of the energy given out by it to the energy put into it; *secondly*, the storing-power of a cell; and, *lastly*, whether or not there was a deterioration in its working-powers. To measure the energy put into any electric circuit, we have merely, of course, to take time-readings of the current flowing through the circuit, as well as the difference of potentials between its two extremities. The current in ampères multiplied by the electromotive force in volts and by 44·25, gives the number of foot-pounds per minute that is being put into that part of the circuit as electric energy. For measuring the current we have used throughout our ammeters (short for ampère-meters), and for measuring electromotive force our voltmeters, the latter being employed of course in a shunt circuit.

Of the total electric energy put into the circuit, and which is measured, in foot-pounds per minute, by 44·25 AV, a portion will be employed simply in heating the circuit, and the remainder may be utilized in producing useful work. For example, if a time-curve be drawn for 44·25 AV when charging a Faure accumulator, the area of the curve will measure the total energy put into the accumulator in foot-pounds; but of this some portion has been wasted in heating the cell, due to the charging having been more rapid than was absolutely necessary. It was, of course, of considerable importance in our experiments to ascertain what portion of the energy put into the cell was really thus wasted; and to measure this the following experiments were made.

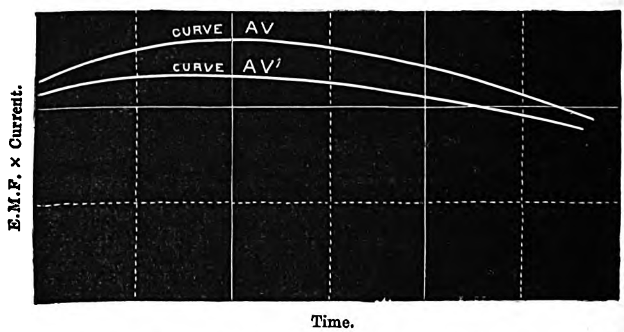
Occasionally the main current was stopped, the shunt current through the voltmeter being left completed. The reading now on the voltmeter gives the difference of potentials produced by the cell itself, whereas the previous reading was the combined difference of potentials produced by the cell and the dynamo-machine charging it. If now a new time-curve be drawn in which the ordinates represent the product of 44·25 AV′, where V′ is the electromotive force of the cell measured on the circuit being broken, and A is the mean value of the current flowing just before breaking and just after

K 2

closing the circuit, the area of the new curve will represent that portion of the energy put into the cell which is usefully employed in chemical decomposition. The difference between the areas of these two curves represents, then, the amount of energy wasted in heating the cell in foot-pounds.

Again, on discharging the cell, experiments of a similar nature have to be made. The product 44·25 AV represents the number of foot-pounds of work per minute the cell is producing in the external circuit, V being the difference of potentials between the two poles of the cell while it is discharging; but, in addition, there is a certain amount of energy which is being expended in heating the cell itself during discharge.

This, as before, may be ascertained by breaking the main circuit, leaving the shunt-voltmeter circuit completed. The reading on the voltmeter V′ now indicates the real electromotive force of the accumulator during discharge; whereas the previous reading, obtained just before breaking the circuit, represents merely the fraction of the total electromotive force employed in sending the current through the external resistance. If a time-curve be drawn with its ordinates proportional to 44·25 AV′, where A is the mean value of the current just before breaking and just after closing the circuit, its area will represent the total number of foot-pounds of energy per minute being given out by the cell; and the difference between the areas of the last two curves will represent the number of foot-pounds of energy employed in heating the cell itself. It is to be noticed that during charging V′ is less than V, whereas on discharging V′ is greater than V.



An examination of thirty-five sheets of time-curves, which

we have drawn from the experiments we made, shows that, in charging, the curve for AV rises at first; and as it rises more rapidly than that for AV′, this means an increase in the resistance of the accumulator.

As the charging continues, the two curves for AV and AV′ approach one another, showing that the internal resistance of the accumulator diminishes again. On the other hand, at the end of a long discharge the curve for AV falls more rapidly than that for AV′, due to an increase in the internal resistance. Now our experiments show a great constancy in the electromotive force of a Faure cell, and that the falling-off in discharging which occurs during a very rapid discharge, or at the end of a long discharge, is due more to an increase in the internal resistance of the accumulator than to a diminution in the electromotive force, which our methods of experimenting above described enable us to separate and measure independently. But, whether discharging rapidly or whether discharging slowly, there is a most curious resuscitating-power in the cell, which, if disregarded, will cause totally erroneous underestimates to be made of the efficiency of the cell.

This resuscitating-power is more marked for rapid discharges than for slower ones. In the case, for example, of an extremely rapid discharge, we found that when the flow had become apparently so feeble that the cell appeared totally discharged, leaving the poles of the cells insulated caused three times as much electric energy to be given out all together in the second discharge as had been given out in the first. And even when several days are taken to discharge the cell—and we may mention that we have had continuous observations made day and night for several days in certain cases—this resuscitating-power is wonderfully marked. An insulation of a few hours will cause the energy given off per minute on redischarging to be eight to ten times as great as it was before insulation. Indeed on one occasion, after a cell had apparently nearly discharged itself, it was left shortcircuited with a thick wire for half an hour, then insulated all night, when the number of foot-pounds of work per minute given off at the commencement of the discharge the following morning was found to be ten times as great as it was on the previous evening, and a greater amount of energy was actually taken from it in the

second discharge than in the first. This phenomenon gives the Faure accumulators a great value for tramcar propulsion, since, as is well known, it is just on starting after stopping that the strain on the horses is so great.

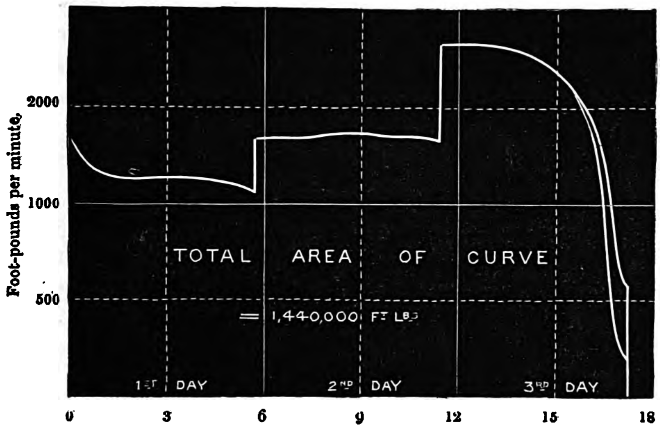
Efficiency.—To determine the efficiency of cells we commence with them empty, or at least as empty as many hours' shortcircuiting with a thick wire could make them. We then measured the total amount of energy put in and the total amount subsequently given out, and we found that, for charges up to a million foot-pounds put into the cell and discharged with an average current of 17 ampères, the loss in charging and discharging combined may not exceed 18 per cent. Indeed, for very slow discharges the loss in charging and discharging combined in some of our experiments has been as low as 10 per cent.

Storing-power.—It is a little difficult to measure the maximum storing-capacity of the cell at the same time that measurements are made of its efficiency, because in the latter case we must take care that we do not put in more electric energy than the cell can hold; on the other hand, if precautions are taken to avoid overcharging, it is a little difficult to ensure that the full charge has been put in. We have therefore separated our experiments for measuring the efficiency from those employed to ascertain the storing-power.

Let us take a single example of the storing-capacity. A certain cell containing 81 lb. of lead and red lead was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot-pounds of work. This is equivalent to one horse-power for three quarters of an hour, or 18,000 foot-pounds of work stored per pound weight of lead and red lead. The curve shows graphically the results of the discharge.

Horizontal distances represent time in minutes, and vertical distances foot-pounds per minute of energy given out by the cell, and the area of the curve therefore the total work given out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging

with a current of about 25 ampères. And this cell, like the others, showed, on being insulated after having been apparently



totally discharged, that there was still a large charge stored up; hence the numbers given above for the capacity are probably under the total value.

Deterioration.—As to deterioration, two months constant charging and discharging of the two accumulators under test showed no signs of deterioration.

XIII. *A Simplified Dispersion-Photometer.* *By* Professors W. E. Ayrton *and* John Perry*.

It will be in the recollection of the Members that in 1879 we described to the Society a dispersion-photometer which enabled measurements to be made of the intensity of the strongest electric light in a small room and for the rays coming from the electric light at any angle—two essentials which appeared to us necessary in an electric-light photometer. The principle of this photometer consisted in our use of a concave lens to weaken the strength of the light, so as to make the illumination of a screen comparable with the illumination of a standard candle, instead of keeping the lamp a distance of 50 or 100 feet away, which was the plan in use until that time.

* Read February 25, 1882.

We exhibit now five successive forms of the instrument, which illustrate the history of its development to the present time.

1. The first of these is very nearly the same as that described in our former paper, with the exception that we discarded the use of a long screw (shown in our original figure) for adjusting the position of the lens—as we found that a very easy adjustment might be effected with the fingers, the tension of the bellows part making an automatic clutch which fixed the lens-slide in any position.

2. The second specimen is on the same principle, only that telescope-tubes are used instead of a wooden frame and a bellows. Instead of the lens part alone tilting when the elevated or depressed light has to be examined, the candle-box is here made to tilt also, the candle being supported in gimbals so that it may remain vertical for every angle of elevation.

3. The third specimen is on pretty much the same principle; but as we found a difficulty in comparing two illuminated disks whose centres were some distance apart, we arranged in front of these disks two mirrors, which enable us to make the comparison between two illuminated semicircles having the same diameter. The difficulty of adjusting the lens and making a comparison of the illuminations, and reading the scale, without moving one's head, in all these early instruments led us to the

4th form, which is probably familiar to the Members, as it was exhibited at Paris and largely used there for measurements. In this the candle-box and the lens-box are placed end to end, the lens is fixed in a wooden piston which moves in its hollow square box, which is lined with velvet; and the lens shows its position by a pointer moving over the scale outside. The pointer projects from the inside of the wooden cylinder at any point of a long slot, whose sides are made of india-rubber tubing, so that no extraneous light can reach the illuminated screen. A little handle working a rack and pinion enables the lens to be placed in any position. Through a hole at the side the two screens can be viewed reflected in two mirrors, inclined to one another in the space between the candle-box and the lens-cylinder; and the illuminated papers are viewed as two semicircles having a common diameter. In front of this hole we have slides of red and green glass; so that,

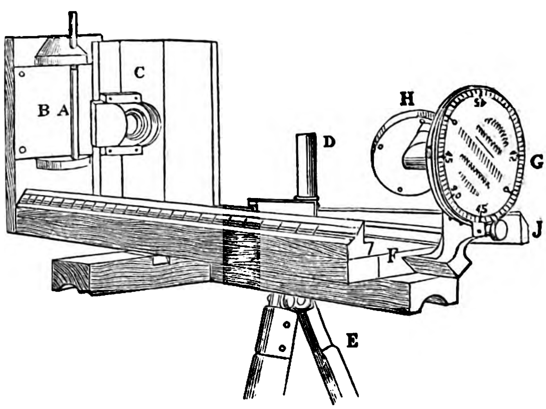
as our custom has always been, we make two measurements—one a comparison of the ruby-red light of the lamp examined with the red light of the candle, and another of the green lights. This instrument differed from the earlier forms in not requiring any calculation to be made of the strength of the light; that is, the reading of the pointer was not merely a reading of its distance from the screen, but it was a reading in standard candles of the power of the light. Three such scales were placed on the instrument; and there were three certain distances at which the lamp had to be placed for examination. The tilting-arrangement was of course different from that of the earlier forms.

As the instrument had by this time (the end of last year) come into a rather extensive practical use, we had opportunities of seeing that, as an instrument to be used by unscientific persons, it was not yet in a perfect condition, in spite of the many changes that had been made in its construction. The most important difficulty was due to the fact that a slight lateral change in the position of the observer's eye caused the apparent illumination of the screens to vary. Being aware of this fact ourselves, we maintained a certain fixed position of the eye when making observations; but the instrument could not at once be used by persons not accustomed to make delicate experiments.

5. The fifth form, which we now present to the Society, is the outcome of our labours on this subject. We have all along seen the disadvantage of using the Bouguer's two-screen method, since, when lights are examined that have passed through tissue or tracing-paper, a very slight change in the position of the observer's eye makes a very great difference in the apparent illumination, whereas, using Rumford's method, when a sheet of white blotting-paper is employed as a screen very considerable changes in the position of the eye produce no change in the apparent illumination—a result, however, which is not attainable when ordinary drawing-paper is used as the screen. If, however, Rumford's method is to be used to measure the rays coming at different angles from an electric light, a mirror must be employed to reflect them successively onto the same screen; and if used in the ordinary way, the angle of incidence of the rays on the mirror will be

different in different cases. Now the difficulty that always met us arose from the inequality of the reflecting-power of an ordinary mirror for rays falling on it at different angles of incidence. We have, however, completely overcome this difficulty in an extremely simple way, by causing the mirror to turn about a horizontal axis inclined at 45° to its plane, and the whole photometer to turn about a vertical axis. With this arrangement the angle of incidence, and consequently the proportional absorption, is the same whatever be the inclination of the rays coming from the lamp to the mirror; and, further, the angle being 45°, the amount of rotation of the mirror about its horizontal axis necessary to enable measurements to be made of rays coming at any angle, after measurements have been made of the horizontal beam, is exactly equal to the inclination of the beam in question.

Using Rumford's method in this latest form of our photometer, we are to a great extent independent of the presence of other sources of illumination of the screen, so that the apparatus need not be enclosed in a box. At the same time, however, the sensibility of the test is much increased by placing a shade to prevent the electric light shining directly onto the screen. On this screen of blotting-paper, B, is thrown the shadow of a black rod, A, placed in front of it, by a candle in the candle-holder, D.



Now it is well known that if an electric light is also allowed to illuminate this screen, and to throw a second shadow of the

rod A on the paper, and if the candle is adjusted at such a distance that the two shadows are of equal intensity, the strength of the light is to that of the candle in the ratio of the squares of their distances from their respective shadows. But instead of allowing the strong light to pass directly to the screen, we cause it to pass through the concave lens in the sliding wooden frame C. A pointer on this slide tells the distance of the lens from the screen. As you are all aware, the weakening of light-intensity produced by the lens enables us to leave our electric lamp within a few feet of the instrument. We have experimentally found that there is no appreciable loss of light in passing through the lens. The candle slides on the bar J; and its distance from its shadow is shown by a pointer on a scale. If f is the focal length of the lens, D the distance of the electric light from the paper-screen, d the distance of the centre of the lens from the screen, and c that of the candle when the shadows show equal illumination, then, if L is the strength of the examined source of light in standard candles,

$$\sqrt{L}=\frac{D-d}{c}\left\{1+d\left(\frac{1}{f}+\frac{1}{D-d}\right)\right\},$$

or

$$L=\frac{1}{c^2}\left\{D+\frac{d(D-d)}{f}\right\}^2.$$

For our own use we prefer to employ the formula; but as all the common instruments which have hitherto been manufactured have lenses whose focal length is 4 inches, we have prepared a table, a copy of which is sent out along with each instrument, in which the value of L is given for various values of D, d, and c. Using this table, it is necessary to have the lamp at either 60, 120, or 300 inches from the screen; the candle is either at 10, 14·14, or 20 inches from the screen; and the table is made out for every half inch of the lens-scale. But inasmuch as we find that the improved arrangement of the mirror already referred to constitutes perhaps the most useful part of the instrument, and as the use of this improvement involves many alterations of D, the manufacturer proposes in future not to furnish any table of values of L unless specially asked for.

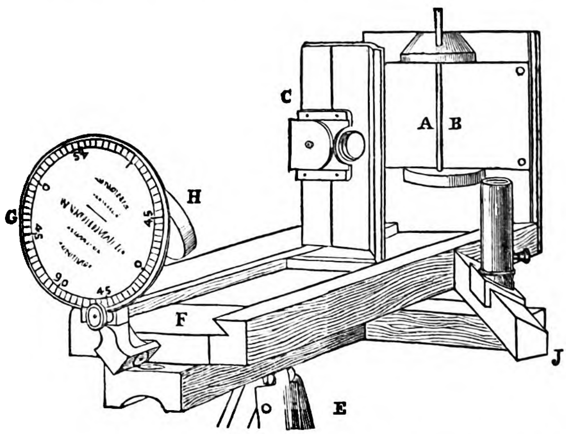
H is the plane silvered-glass mirror which makes the angle

of 45° with the axis of the lens, and with the axis about which the mirror itself is free to revolve. As already explained, a ray of light reflected from the mirror and passing through the centre of the lens must, for any position of the mirror, have an angle of incidence of 45°, and so must experience the same amount of absorption, from whatever direction it may have come to the mirror. Further, this angle being 45°, a fixed pointer marks on the graduated circle G, which moves with the mirror, the angle which any ray we may be examining makes with the horizontal.

In this instrument we find that from 30 to 34 per cent. of the incident light at 45° is absorbed, whether this light is of ruby-red or signal-green colour; so that we have the easy practical rule for all cases—add one half to the measured intensity of light reflected.

We need not here refer to the fact that, when investigating the efficiency of an electric lamp, we always measure the horse-power given electrically to the lamp simultaneously with the photometric measurement.

The lamp is suspended in such a way that it can readily be placed at any elevation. The frame of the tripod-stand is first levelled. A pin at F, directly underneath the centre of the



mirror, passes through the base of the photometer and fits into a hole in the top of the tripod-stand. The photometer, by turning round this pin, can, without producing any change in

the distance of the centre of the mirror from the lamp, and therefore without changing the distance from the screen to the lamp, receive the small horizontal motion necessary for the adjustment of a new inclination of the rays coming from the electric light, without any alteration of the distance of the centre of the mirror from the lamp. The divided circle is clamped with the index at 0°; the lamp is lowered or raised till the illuminated disk formed by the reflected light, passing afterwards through the lens, is in the middle of the paper screen. A little sliding shutter with a fine hole in its centre, seen in the figure, enables a very exact adjustment to be made; but in practice we find that we get sufficient accuracy without the use of the shutter. We now measure the distance from lamp to centre of mirror in inches. Equalizing the intensities of the two shadows by adjusting the lens-slide when looking at them through red or green glass, we now note the lens- and candle-readings; and we repeat these operations, changing from red to green and green to red about five times in a minute. The lamp is now raised or lowered and fixed in any position; a few seconds suffice to turn the mirror so that it sends its centre ray exactly through the centre of the lens. The distance from screen to mirror in this instrument being 22 inches, if δ is the distance from centre of mirror to vertical from lamp, and if θ is the angle of elevation, then

$$D = 22 + \delta \sec \theta.$$

Using this value of D in the formula above, and adding one half to the strength of the light to make up for absorption, the true intensity of the light in standard candles can be ascertained. We find in practice that, if an electric light is moderately steady, ten measurements may be made, with some confidence in their accuracy, in two minutes; and the light may be measured in ten different positions, from an angle of depression of 60° to an angle of elevation of 60°, 100 observations being taken, in less than half an hour.

We may mention one very important result we have been led to by the systematic employment of a photometer which can be used close to the electric light; and that is the large amount of absorption that occurs on certain days when the rays from strong electric lights, and especially the green rays, pass through the air which appears to the eye perfectly clear.

At first we were inclined to think the higher results for the candle-power of a lamp obtained with our dispersion-photometer than those obtained with an ordinary distance-photometer were due to some error in our photometer itself; but we have since ascertained that this is due to the absorption of the air—because we find that, if simultaneous measurements are made with ordinary Rumford's photometers, each without lens or mirror, placed at different distances from the lamp in the same azimuth and in the same horizontal plane, the nearer one gives, as a rule, the highest readings; and the difference is the greater the stronger the light, and is greater if the light be examined at each photometer with green glass.

XIV. *Notes on Thermometry.* *By* F. D. Brown, *B.Sc.*, *Demonstrator of Chemistry at the University Museum, Oxford.*

[Plate VI.]

Some years ago, when I determined to try and find out something about the attractive forces which the atoms and molecules seem to possess, by studying the effects of heat upon chemical substances and upon mixtures of such substances, I was led to the conviction that, if the work which I proposed to do was to be of any permanent use, I should be obliged to take many and minute precautions regarding the measurement of temperatures—a measurement which, owing to the peculiarities of mercurial and other thermometers, is so liable to error. In order to learn how best to use my thermometers, and how to refer their readings to a satisfactory standard, I made a considerable number of experiments. At the time when these experiments were made I imagined that the subject of thermometry, although presenting many difficulties to my mind, had been thoroughly worked out by others, and therefore that a printed record of my observations would be generally deemed to be of little utility. The recent publication of a paper by Dr. E. J. Mills (Edin. Roy. Soc. Trans. 1880), of one by Professors T. E. Thorpe and Rücker (Phil. Mag. [5] xii. p. 1), and more especially of a report by M. Pernet (*Mém. et Travaux du Bur. inter. des poids et mes.* i. 1881, pp. 1–52), has led me to change my opinion, and to

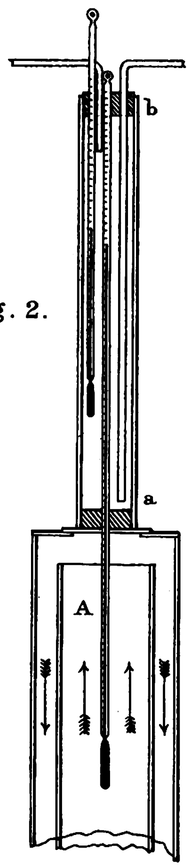


Fig. 2.

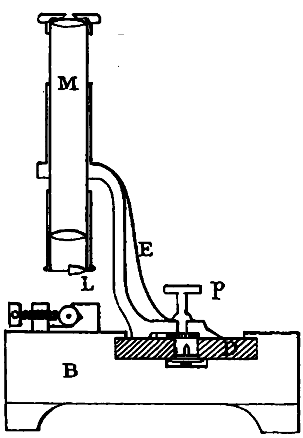


Fig. 1a.

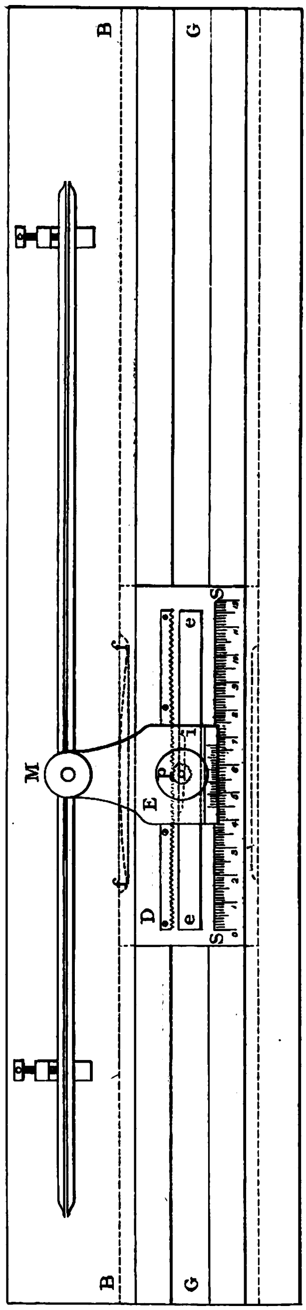


Fig. 1.

Mintern Bros. lith.

think that there still remain many points connected with thermometers about which not only I, but others also, would be glad to have more certain information. Acting upon this belief, I have put together in the following pages some of the results of my experiments.

The Mercurial Thermometer as a Standard.

I was soon convinced that any attempt to express temperatures in degrees of an ideal absolute thermometer, or even to refer them correctly to the readings of an air-thermometer, would involve a most extensive and wearisome investigation, which would postpone indefinitely the work I wished to do. To avoid this substitution of the means for the end, I decided to construct a mercurial thermometer and to use it as a standard, keeping it until such time as the progress of our knowledge should render its comparison with the air-thermometer a matter of less difficulty.

As a mercurial thermometer is very liable to be broken, I first wanted to know whether this instrument fulfilled the primary condition of a true standard, of being capable of reproduction when lost or destroyed. With this end in view, I made two thermometers at different times, and wholly independently one of the other, and compared their readings. To those who may wish at any time to construct a mercurial thermometer without the elaborate appliances ordinarily employed, but in which absolute confidence may be placed, the following details may be of interest:—

A capillary tube of medium bore, about 800 millimetres long, free from all flaws, and having as uniform a section as possible, is provided with a millimetre-scale of 600 divisions. The etching of this scale is a matter of great consequence: it very frequently happens that the divisions on glass tubes are not of exactly equal length, but that, owing to some defect in the dividing-engine or some movement of the tube while undergoing the process of division, some of the divisions are so much longer or shorter than the rest as seriously to interfere with the subsequent process of calibration. Even when all the lines are equidistant, they are often so thick, and present so irregular an outline when viewed through a telescope, that it is impossible to fix upon any particular point as that repre-

sented by the dividing-line. The tubes I employed were selected and divided with special care by Mr. Casella, the lines being perfectly straight, less than 0·4 millim. in thickness, and in all cases equidistant.

As a glass tube, however carefully selected, is never of uniform bore, it is necessary to ascertain the relative capacities of the several divisions of the tube, or, in other words, to "calibrate" it. As is well known, this is easily done by placing a thread of mercury in successive positions along the tube and observing its length, the mean capacity of the divisions occupied by the thread being, of course, inversely proportional to that length. In this way, and by adopting the plan of correcting the position of the thread suggested by Dr. Mills in the paper above referred to, which plan he had been kind enough previously to communicate to me privately, a table is readily constructed showing the volume of the tube from the line marked 0 to any line marked *n*, and also the value of the succeeding division. The only difficulty connected with this process is the accurate measurement of the length of the thread of mercury in its several positions. It is true that this may easily be done with a dividing-engine or some similar instrument, such as a cathetometer provided with a micrometer eyepiece and placed horizontally. As, however, reliable instruments of this class are exceedingly costly, I designed a small piece of apparatus for the purpose, which has proved so convenient and useful that I venture to describe it here.

A mahogany board, BB (Pl. VI. fig. 1), about 18 inches long and 4 inches wide, is provided with a groove, G G, of the shape shown in the section (fig. 1 *a*); a piece of gun-metal, about 5 inches long and ¼ inch thick, slides in this groove with some little difficulty—the friction, which is produced by the spring *f f*, being necessary to retain the plate rigidly in any given position. The plate, D, is provided with a slot, *e e*, and a millimetre-scale, S S, the dividing lines of which must, like those of the tube to be calibrated, be very fine and truly equidistant. The piece of gun-metal, E, which is provided with a vernier, carries the reading-microscope, M, and can be moved along S S by means of the rack and pinion *p*; the movement is rendered smooth and free from lateral displacement by the spring *c*, which causes the ends of E to remain always in contact with the

straight edge of the slot. The thermometer-tube is fixed with suitable screws under the path of the microscope, so that the length of a thread of mercury can be easily measured by placing the microscope so that its cross wire coincides first with one end of the thread and then with the other, and noting on the scale the distance between the two positions.

The millimetres of the brass scale and those of the tube, if marked off by different makers, will often differ a little in length; hence it is generally more satisfactory to obtain from the glass scale the number of whole divisions occupied by the thread, and to measure the terminal fractions only by the microscope.

Since the line on the outside of the tube is nearer the eye than the thread of mercury inside the tube, it is clear that when the microscope is adjusted to view the end of the thread, and is then moved along until the cross wire coincides with the nearest line, this last will be out of focus, and either the whole microscope must be raised up or the distance between the object-glass and eyepiece altered. Now, unless the instrument be constructed with great solidity, and much care be taken to fit accurately all the moving parts, this adjustment will probably alter the position of the optical axis, and so render the measurements inaccurate. To avoid this difficulty, I added a half-lens, L, fitted in the ordinary way on a brass tube sliding on the end of the microscope. This lens of course brings the focus of half the field nearer the object-glass; so that, by properly adjusting it, the divisions are seen through the half-lens at the same time that the mercury is observed through the unprotected part of the object-glass. In this way all disturbance of the microscope is avoided throughout the calibration, which is thus carried out with much greater comfort and accuracy.

Two tubes were calibrated with this apparatus, and tables of their volumes from the first division compiled; they were then furnished with bulbs, filled with mercury, and sealed up in such a manner that they formed thermometers capable of indicating temperatures between 0° and 150° C. The fixed points of the two thermometers having been determined with the precautions indicated below, tables showing the temperatures corresponding to the readings of the scale were

made in the usual manner; the two instruments were then compared together, either in a large tank of water which was kept well stirred, or in the steam-apparatus which I described to the Physical Society at the time when these experiments were made. Before a series of readings were taken, both thermometers were heated for at least half an hour in steam, while their zero-points were observed after the series was completed. The numbers given in the following table show that the two thermometers gave practically identical readings. It would seem, therefore, that the mercurial thermometer, when carefully made and systematically heated, does really possess that valuable property of a standard, of being capable of exact reproduction.

Reading of AS, corrected for index-error.	Reading of BS, corrected for index-error.	Corresponding value of AS, in degrees.	Corresponding value of BS, in degrees.	Difference.
58·55	70·64	14·30	14·29	−·01
134·33	150·11	33·69	33·71	+·02
179·69	197·20	45·29	45·30	+·01
321·97	345·96	81·88	81·88	·00
23·42	33·74	5·28	5·28	·00
26·33	36·83	6·03	6·04	+·01
30·07	40·63	6·99	6·97	−·02
33·76	44·57	7·94	7·93	−·01
43·32	54·70	10·40	10·40	·00
47·98	59·60	11·59	11·59	·00
69·42	82·13	17·09	17·09	·00
91·61	105·52	22·78	22·79	+·01

Determination of the Zero-point.

In most books on physics it is stated that, in order to obtain the zero-point of a thermometer, the instrument should be placed in a vessel filled with broken ice and provided with holes at the bottom, through which the water formed by the melting of the ice may escape. In order to learn whether this method is the best possible, the following experiments were made:—A number of tin pots, about 7 inches high and 4 inches in diameter, were obtained, and holes made in the bottoms of two or three of them. A large block of ice was broken up into small fragments, which were well mixed up, so as to render the whole perfectly uniform in character. One of the tin pots, which we will call A, was filled with

some of this ice, which had been washed in a funnel with ordinary water; A was then filled up with water, so as to form a mixture in which the ice largely predominated. A second tin, B, was filled with some more of the ice, which had been washed with ordinary water in the same way; B, however, had holes at the bottom, and the water formed by the fusion of the ice thus drained away. A third tin, C, contained some of the same ice, which had been washed in a funnel with distilled water, and then mixed with distilled water in the same way as in A the ice was mixed with ordinary water. In a fourth tin, D, which was provided with holes, some ice was placed which had been washed with distilled water. Finally a quantity of distilled water was artificially frozen the ice broken up into small pieces, washed, and mixed with distilled water in a fifth tin, E. A thermometer with a long narrow bulb, and with a stem divided into millimetres, was carefully inserted into each tin in succession, and readings taken with a cathetometer. About 17 millim. of the scale were equivalent to one degree Centigrade. In A the readings soon became constant at 1°·00; in B the readings varied considerably for about half an hour, but finally became constant at 1°·12; in C the thermometer became rapidly constant at 1°·16; in D the readings became constant after a short time at 1°·06; in E the readings did not vary after the first four or five minutes, remaining at 0°·64.

At the end of these observations, which occupied nearly two hours, the thermometer was replaced in A, where the mercury rapidly assumed the same position as before, viz. 1°·00. Seeing that, with the exception of E, the greatest difference in the readings does not amount to 0°·01 C., we may fairly draw the following conclusions:—First, that a constant temperature is more rapidly and certainly obtained with a mixture of ice and water than with ice alone; secondly, that the temperature thus obtained is really that of melting ice; thirdly, that it is preferable to wash and mix the ice with distilled water, ordinary water tending to lower the temperature, though to an insignificant extent.

With the view of seeing whether different varieties of ice gave the same results, two specimens of block ice and one of the rough thin ice collected in winter near London were

obtained, while two cylinders of distilled-water ice were artificially produced. These were all broken up separately into small pieces, washed with distilled water, and then mixed with the same in five tins, A, B, C, D, E. The thermometer placed in these tins marked 1°·30, 1°·34, 1°·26, 1°·30, and 1°·27 respectively (these numbers are not comparable with the former, as the experiments were made a month or so later, when the zero of the thermometer had altered its position). These experiments showed that distilled-water ice gave the same results as ordinary ice, and that the melting-point of different specimens of ice, when mixed with distilled water, was the same within 0°·005 C. The exceptionally low reading obtained with the tin E in the first series of experiments was probably due to the fact that the ice, having been made by means of a freezing-mixture, was not at its maximum temperature.

In subsequent determinations of the zero of thermometers I have always used ordinary block ice, washed and mixed with sufficient distilled water just to fill up the spaces between the pieces, and have not allowed the water to drain away. These results are in accord with those obtained by M. Pernet.

Zero-movements, and Substitution of the Determination of the Steam-point for that of the Zero-point.

In considering the well-worn question of the zero-movements of thermometers, it is important to distinguish between its practical and theoretical aspects. To make a study of zero-movements from an abstract point of view, to find out equations expressing these movements under different circumstances and with different thermometers, to learn that when a certain thermometer has been subjected to a certain series of temperatures at certain intervals of time its indications on next changing its temperature will be affected with a certain index error, may possibly be of some utility, but it does not aid us much in the endeavour to free the readings of thermometers from the errors with which they are surrounded. When once we have acquired the information that a thermometer subjected only to those changes of temperature which are due to the weather exhibits a gradual rise of zero, that the rise thus taking place in a given time diminishes as the age of the thermometer increases, but differs for different thermometers, when

we also know that a thermometer subjected to a high temperature after a considerable period of rest exhibits a decrease in its zero-reading, dependent on the thermometer itself and also on its previous history,—we know all, or nearly all, that we can put to practical use.

Thus, for example, the thermometer attached to my standard barometer was verified at Kew Observatory when it was first supplied to me, some four or five years ago. Since then I have from time to time observed its reading in melting ice, and have modified accordingly the correction to be applied to it. Now, no observations of other thermometers—no curves or equations representing their zero-movements—could be of any assistance to me in this matter. I knew that the zero would probably rise, and that the amount of the rise would not be the same in my case as in that of others, and that, therefore I must obtain the index-error experimentally. I also knew, that if I boiled the thermometer I should cause irregular changes in the position of the zero; and as there was no necessity for the operation, I avoided boiling it. But if by mischance it had fallen in boiling water, no equations representing the zero-movements of other thermometers would have told me exactly what had happened to mine; I should simply have been obliged to observe its index-error more frequently than before the accident happened.

The question which seems to me to be of the greatest importance with regard to zero-movements is, how we can best reduce the trouble which they cause us. In the case of all meteorological and clinical thermometers, where the changes of temperature are small, as in the above case, it is evident that all we can or need do is to protect the instrument from unnecessary changes of temperature. When, on the contrary, our observations extend over wide ranges of temperature, the difficulties increase considerably. Suppose, for example, that I want to use a thermometer to indicate accurately a series of temperatures between 70° and 90°. It is obvious that if I observe the index-error beforehand, and apply the correction thus obtained to my readings, I shall not be doing right; for the very heating of the thermometer to 70°–90° will have altered the index-error. But if, on the other hand, I first heat the thermometer to 100°, then ascertain its index-error, then make my experi-

ments with it, and finally observe its reading in ice a seecond time, I shall be tolerably certain, if the index-error is the same at the end as at the beginning of the experiment, that no variation has occurred during the observations.

In most laboratories, however, the frequent determination of the zero-point of a thermometer involves a considerable expenditure of labour: ice has to be purchased, broken up into small pieces, washed, and placed in a suitable vessel. All this requires no little time, and has, moreover, to be repeated at every determination, since the broken ice melts away in the interval. On the other hand, the apparatus for the observation of the steam-point is always in readiness; if, therefore, no greater error arises when the index-error is determined before and after the experiments by means of the steam-point, a great saving of time will be effected, without any corresponding loss of accuracy.

When the temperatures to which the thermometer is to be exposed are greater than 100°, the instrument should be heated for some time to the highest probable temperature before the steam-point is observed for the first time. In this way the lowering of the zero which takes place when a thermometer is heated from 100° to some higher temperature, to which it has not been exposed for some time previously, is effected first of all, and does not take place during the experiments, as it otherwise would.

The only objection which can be raised to this method is that, when some at least of the temperatures to be measured are below 100°, it is possible that the steam-point, which is lowered by the first heating in steam, rises again during the experiments (that is, when the thermometer is at a lower temperature), and then, by the second heating in steam, is again brought to the same position as at first. In this way the observations in steam, although concordant, would not give the true index-correction to be applied to the readings. That the error which thus arises is of no importance is, I think, rendered probable by the following considerations:—The gradual rise of the zero of a thermometer receives its most natural explanation when it is supposed that the glass bulb, after having been heated and somewhat quickly cooled, is in a state of strain which causes it to have a larger capacity

than it would have if no such strain existed. As time goes on, and more especially as the thermometer is subjected to small fluctuations of temperature, the particles of the glass gradually yield to the forces which are acting upon them, and take up new and more suitable positions. These molecular movements result in a gradual diminution of the capacity of the bulb, and consequently in a rise of the zero. Now it is evident that, if a certain state of strain is set up when a thermometer is cooled from 100° to 0°, when it is cooled from 100° to some intermediate temperature *t* the strain set up will be less considerable; there will therefore be a greater tendency for the zero to rise when the thermometer is placed in melting ice than when it is subjected to the temperature *t*. Consequently, if it be found that, when a thermometer after being heated in steam is placed in ice, no change of the zero takes place for three or four hours afterwards, we may legitimately conclude that, if the thermometer were maintained for the same time at the temperature *t*, no movement of the zero would occur. I have frequently kept recently-heated thermometers in melting ice for several hours, renewing the ice when necessary; and I have always observed, with all of my instruments, that no change took place for the first three hours, and that during the next two or three hours the rise was extremely small. It follows, therefore, that if in any series of observations lasting more than three hours the thermometer be heated in steam at the end of every third hour, there will be no uncertainty as to the position of the zero; that if the experiments be carried on continuously for six hours, a slight rise of the zero may occur during the last part of the time, but that this rise will not amount to more than one or two hundredths of a degree.

Correction for the Exposed Portion of the Thread.

When a thermometer is only partially immersed in the medium of which the temperature is to be observed, the readings become subject to an error which arises from the fact that a part of the thread of mercury, together with the corresponding portion of the stem, are at a temperature different from that of the bulb and immersed portion of the stem. The correction, C, usually applied in this case is given

by the formula

$$C=m(T-t)N, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where T = the reading of the thermometer,
t = the temperature of the exposed portion,
N = the number of exposed divisions of the stem which are filled with mercury,
m = the apparent expansion of mercury in glass.

This formula is founded on the assumption that the error in the reading has no other cause than the comparatively unexpanded condition of a portion of the thread and stem.

The apparent expansion of mercury in glass, as obtained from Regnault's experiments, is about ·0001545; but it differs, of course, for different specimens of glass. When this number is employed in the above formula, the values of C obtained are generally believed to be too large; indeed a little reflection will convince us that this must be the case whenever the temperature of the exposed portion is merely measured by placing another thermometer with its bulb halfway up it. This second thermometer evidently measures the temperature of the ascending stream of warm air around the stem; if the stem of the chief thermometer were subjected to the heating influence of this stream, and to no other, its temperature would be rightly given by the subsidiary thermometer; but the thermal conduction along the thread of mercury and along the glass stem must necessarily raise the lower part of the exposed stem to a temperature higher than that indicated by the subsidiary thermometer. The value of $(T-t)$ therefore is too great, and consequently also that of C.

In order to meet this difficulty, Dr. Mills, instead of endeavouring to give to $(T-t)$ its proper value, has made a large number of experiments with different thermometers with a view to assign a more satisfactory value to m, and has thus been led to draw the following conclusions:—The value ·0001545 of the coefficient m is invariably too great. This coefficient varies with the thermometer employed, and also with the number of divisions of the thread exposed; so that, instead of assigning one definite value to m for each thermometer, we must give it a value

$$m=\alpha+\beta N,$$

where α and β must be determined for each thermometer.

Professors Thorpe and Rücker, on the other hand, while admitting that the value $m = \cdot 0001545$ may be generally too large, maintain that it is sufficient to replace it by some other single number, and that the employment of the varying coefficients $\alpha + \beta N$ is unnecessary; they support this opinion by showing that in Dr. Mills's own experiments the alterations in the value of C, caused by the introduction of the term βN, do not amount to more than one or two hundredths of a degree, and are therefore insignificant. Dr. Mills, replying to this, states that the change in the correction C brought about by the term βN often amounts to so many hundredths of a degree that it cannot be neglected.

Now it is clear that by merely placing a second thermometer halfway up the exposed thread, only the roughest idea is obtained of the real temperature of the thread. Suppose, for example, that $T = 100°$, and that t is taken at 15°, being subject to an error of 5°: the value of $(T - t)$, which is 85, will be subject to an error of 5°, or about 6 per cent. What, therefore, can be the use of attempting to determine the coefficient β, of which the value would appear ordinarily to be about 0·0000002, when so great a source of error is left unprovided for?

In all experiments in which I have had occasion to use mercurial thermometers, I have endeavoured to avoid any correction for the exposed thread, by making the apparatus and thermometers employed of such relative dimensions that the whole thread and bulb, except the topmost division, are at the same temperature. When this is impossible, and when the experiments require such extreme accuracy, it seems to me that the first thing to be done is to surround the exposed portion of the thread with a current of running water, and so, while preserving it from the uncertain effects of conduction, radiation, &c., to render possible the observation of its exact temperature. The value of $(T - t)$ being thus correctly measured, that of m is found to be constant for all values of N, and to differ but little from 0·001545. It varies, however, with different thermometers.

The following experiments show most distinctly the truth of this statement:—

One of the standard thermometers mentioned in the first

section of this communication was partially surrounded by a glass tube, *a b* (fig. 2), about an inch in diameter; this tube was closed at the bottom with a piece of good cork, about 8 millim. thick, through which the stem of the thermometer passed. The upper end of the tube *a b* was fitted with a cork, in which were four holes—one for the stem of the chief thermometer, a second for a thermometer to indicate the temperature of the water contained in the tube, while through the two others passed the tubes by means of which the current of water was maintained. The thermometer thus furnished was fixed vertically in the ordinary apparatus, A, for determining the 100°-point of thermometers. The open end of A was closed with a thin disk of brass, with a small central hole, through which the thermometer passed. One degree was equal to about four divisions of the millimetre-scale of the thermometer, the readings of which were observed with a cathetometer, and the fractions of a division measured with that instrument. It was found that the readings of the thermometer under these conditions were correct to ·02 of a millimetre, or ·005 of a degree. The numbers given below are the means of three readings, which, however, were nearly always identical. The thermometer in the water was graduated to fifths of a degree, and had been compared with the standard.

The chief thermometer was first heated in the steam for an hour, with two or three inches of the thread above the cork; it was then pushed down until the quicksilver was only just visible above the cork, and the reading noted; it was then pulled up again, and readings taken in various positions, as given in the following table; finally the thermometer was again pushed down as far as possible, and the reading taken, when it was found to be the same as before, showing that no change in the 100°-point had supervened during the experiment. Of several series of observations made in this manner, the one contained in the following table will suffice, since they all led to precisely the same result.

Reading of Standard when wholly immersed $=393{\cdot}42$.

Barometric pressure, corrected and reduced, $=760{\cdot}1$.
Corresponding temperature of steam $=100^\circ{\cdot}00$.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard, T.	Value of C $=393{\cdot}42-\text{T}$.	Value of m, $=\frac{\text{C}}{(\text{T}-t)\text{N}}$.
317	13°·0	389·01	4·41	·0001599
277·5	12·3	389·54	3·88	·0001594
221	12·0	390·30	3·12	·0001604
173	12·1	390·94	2·48	·0001631
130	12·1	391·58	1·84	·0001610
79	12·1	392·30	1·12	·0001612

An inspection of the above table is sufficient to convince us that the value of m is constant, and equal to the apparent expansion of mercury in the glass of which the thermometer was made; the numbers would probably have agreed even more closely, were it not that it is impossible to arrange the apparatus so that the cold portion of the thermometer-stem follows directly upon the hot portion. There must always be an interval occupied by the cork, the temperature of which is uncertain. It should be remarked that there is no indication whatever of the value of m increasing when that of N increases.

Precisely the same results were obtained with the second standard thermometer, as is shown by the following table:—

Reading of Standard BS, when wholly immersed, $=419{\cdot}21$.

Barometric pressure, corrected and reduced, $=760{\cdot}5$.
Corresponding temperature of steam $=100^\circ{\cdot}02$.

Number of divisions surrounded by cold water and occupied by mercury.	Temperature of water.	Reading of Standard BS, =T.	Value of C, $=419{\cdot}21-\text{T}$.	Value of m, $=\frac{\text{C}}{(\text{T}-t)\text{N}}$.
302	12°·0	415·13	4·08	·0001535
237	11·9	415·96	3·25	·0001556
174	12·0	416·84	2·37	·0001548
127	12·0	417·47	1·74	·0001557

Here, again, the value of m varies only within the limits of the error of observation, and shows no tendency to increase

when N increases. It may be noted that with both the above thermometers the mean value of m is *greater* than ·0001545, the value usually assigned to it, but that it differs from that number by so little that the error committed by substituting the one for the other in the calculation of the correction C will rarely amount to more than 0°·02 C.

The above experiments were made at 100°, because this is the only temperature which can be maintained absolutely constant for an hour without the use of a quantity of complicated apparatus; and it is evident that the slightest variation in the temperature would entirely spoil the series of observations. At higher temperatures the sources of error which beset the readings of thermometers increase so rapidly that the exact value of the coefficient m becomes of less and less importance as the temperature rises, notwithstanding the fact that the correction C increases in amount. Since there is no reason whatever to suppose that any different results would be obtained at such higher temperatures, I thought it unnecessary to make any further experiments, more especially as those given above yielded precisely those numbers which the ordinary laws of expansion predicted.

There is another point connected with thermometry, to which I devoted attention some years ago. It has been suggested that when a thermometer is placed in a vapour at maximum tension, as in the ordinary chemical process of distillation, it does not truly indicate the temperature of the vapour. This suggestion owes its origin to the fact that drops are seen to accumulate and drop off the end of the thermometer. It has been supposed that this condensation of the vapour on a surface which should be as hot itself, is due to the molecular attraction of the glass for the vapour. If this be the case, the heat evolved by the vapour during liquefaction on the thermometer-bulb would raise the temperature of the latter. The thermometer would thus indicate a higher temperature than that of the mass of the vapour. The experiments which I made upon this subject, like those instituted by others, were inconclusive. I possess, however, an apparatus which seems to me eminently suited to answer the question satisfactorily. It is at present being employed for other purposes; but I trust that, when it is at liberty, I shall be able to put it to this not unimportant use.

XV. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VI. *By* C. R. Alder Wright, *D.Sc.* (*Lond.*), *F.R.S.*, *Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School**.

On the Relations between the Electromotive Forces of various kinds of Cells analogous to Daniell's Cell but differing therefrom in the nature of the Metals used, and the Chemical Affinities involved in the Action of these Cells.

I. *Cells containing Cadmium as one of the Metals, the Salts used being Sulphates.*

118. The experiments described in Part V. (§§ 106–109) were repeated, using, instead of normal Daniell cells, analogous arrangements containing plates of cadmium, opposed in some instances to copper, in others to zinc, solutions of the respective sulphates being employed to surround the various plates used. With each of these two classes of cells (cadmium-copper and zinc-cadmium cells) the same result was obtained as that already recorded in the case of Daniell cells containing zinc- and copper-sulphate solutions—viz. that so long as the two solutions are of the same strength† the actual state of concentration of the fluids does not exert any appreciable influence on the E.M.F. generated with given plate-surfaces; at least the influence exerted is considerably less than the errors of observation and the variations

* Read June 24, 1882.

† It is convenient to define solutions "of the same strength" not as solutions of the kind usually spoken of by chemists as "equivalent" to one another, *i. e.* containing in a given volume quantities of dissolved matter in the ratio of the chemical equivalents of the substances dissolved (*e. g.* 159·5, 161, and 208 parts of anhydrous copper, zinc, and cadmium sulphate respectively), but as solutions in which *the dissolved matter and the water present are in the same molecular ratio*, i. e. which are expressible by parallel formulæ, such as $CuSO_4\ 50H_2O$, $ZnSO_4\ 50H_2O$, and $CdSO_4\ 50H_2O$. With weak solutions the two definitions are practically the same—but not so with more concentrated fluids, especially when the molecular weights of the dissolved matters are considerably different (like $CuSO_4$ and $CdSO_4$). Solutions of zinc and copper sulphate of the same molecular strength are practically identical in specific gravity; but a solution of cadmium sulphate is considerably more dense than one of either zinc or copper sulphate of the same molecular strength. Thus solutions of the strengths $ZnSO_4\ 50H_2O$, $CuSO_4\ 50H_2O$, $CdSO_4\ 50H_2O$ have at 18° specific gravities respectively close to 1·170, 1·167, and 1·208; with stronger solutions the excess of density of the cadmium solution is still more apparent.

due to unavoidable variations in the nature of the plate-surfaces, and does not amount to as much as $\pm \cdot 0015$ volt even when tolerably concentrated solutions of strength $MSO_4\ 50\,H_2O$ are compared with similar solutions of only one twenty-fifth the strength, $MSO_4\ 1250\,H_2O$.

On varying the nature of the surface of the cadmium plate (by employing bright cast metal, electro-deposited cadmium, or amalgamated cadmium), it was found that whatever result was produced in the cadmium-copper cells by a given alteration of the cadmium plate, every thing else remaining unaltered, *precisely the same numerical result, but with the opposite sign, was produced in the zinc-cadmium cells* by that alteration. Thus, substituting electro-cadmium for bright cadmium plates in the cadmium-copper cells caused an *increase* in the E.M.F. varying from ·002 to ·006 volt in numerous experiments, and averaging ·004 volt; whilst with the zinc-cadmium cells the same substitution caused a *decrease* in the E.M.F. varying between almost the same limits, ·002 and ·007 volt, and averaging almost the same value as before, viz. ·0045 volt. Similarly, on substituting amalgamated cadmium plates for bright cadmium in the cadmium-copper cells, the average effect was a *decrease* of ·0415 volt when the mercurial amalgam was fluid, and of ·015 volt when it had become solid and crystalline on standing; whilst with the zinc-cadmium cells, substitution for bright cadmium of amalgamated metal caused on an average an *increase* in E.M.F. of ·043 volt when the amalgam was fluid, and of ·016 volt when it had become solid and crystalline.

Cells containing Cadmium opposed to Copper.

119. On comparing together a number of similar pairs of cells containing in the one case electro-copper and in the other amalgamated copper, it was found that the average difference was sensibly the same as that observed when the same two kinds of copper plates were opposed to zinc (§ 107), viz. that, *cœteris paribus*, the cell containing amalgamated copper read on an average ·001 volt lower than the one containing freshly deposited electro-copper: the actually observed differences ranged from $+\cdot 003$ to $-\cdot 003$ volt, but were more usually negative.

As just stated, when the cadmium plate was amalgamated a decrease in E.M.F. was brought about, averaging ·0415 volt when the amalgam on the surface of the plate was fresh and

perfectly fluid, and ·015 when perfectly solid and crystalline. Intermediate numbers were given by plates on the surface of which crystallization of the amalgam had begun but was not complete, the gradation being regular as the crystallization progressed.

The following table gives the average result, in volts*, of upwards of forty series of observations and comparisons, mostly extending over three to four hours, during which time the readings of each particular cell remained sensibly constant:—

Variation in E.M.F. due to the use of cadmium and copper sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4\ 47H_2O$, to $MSO_4\ 1250H_2O$)† . . . . } Less than ±·0015.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for electro-copper:—				
Amalgamated copper (surface wet with liquid mercury)	+·003	−·003	·006	−·001
Effect of substituting for bright cadmium:—				
Fresh electro-cadmium	+·006	+·002	·004	+·004
Amalgamated cadmium (surface wet with liquid mercury	−·050	−·033	·017	−·0415
Amalgamated cadmium (solid and crystalline)	−·020	−·005	·015	−·015
Electromotive force of combinations:—				
Electro-copper—Electro-cadmium	·756	·750	·006	·7525
„ „ Bright cadmium	·753	·745	·008	·7485
„ „ Amalgamated cadmium (liquid amalgam)	·717	·701	·016	·707
„ „ Amalgamated cadmium (solid amalgam)	·740	·727	·013	7335
Amalgamated copper—Electro-cadmium	·754	·749	·005	·7515
„ „ Bright cadmium	·752	·744	·008	·7475
„ „ Amalgamated cadmium (liquid amalgam)	·715	·701	·014	·706
„ „ Amalgamated cadmium (solid amalgam)	·737	·727	·010	·7325

* All the observations given in this paper are reduced to the same standard as that employed in Part V.—viz. the average reading at 15°·5 of a number of Clark's cells taken as 1·457 volt, the particular Clark's cells used being the same throughout.

† The specific gravities at about 19° of these fluids are close to the following:—

$MSO_4\ 47H_2O$ when M is cadmium: spec. grav. = 1·217.
do. when M is copper: spec. grav. = 1·175; solution nearly saturated.

$MSO_4\ 1250H_2O$: in each case below 1·01.

When cells containing bright or electro-cadmium and electro-copper plates were allowed to stand for twelve hours or longer periods, a slight alteration in the E.M.F., due to formation of films of oxide on the surfaces of the plates, was usually noticeable. As with the normal Daniells (§ 108), the effect of the oxidation of the copper plate was to reduce the E.M.F. by a few thousandths of a volt; on the other hand, the formation of a film of oxide on the surface of the cadmium plate produced an *increase* in the E.M.F. of from ·001 to ·004 volt; so that in many cases the cell with partially oxidized plates gave sensibly the same value as a newly set-up cell, the diminishing effect of the oxidation of the copper being just about counterbalanced by the increasing effect due to the oxidation of the cadmium. In this respect cadmium behaves in the opposite way to zinc (§ 108).

Relations between the E.M.F. of Cadmium-Copper Cells and that corresponding to the net Chemical Action taking place therein.

120. According to Julius Thomsen's determinations (*Journ. prak. Chem.* ii. p. 233, and xi. p. 271), the heat of displacement of copper from copper-sulphate solution ($CuSO_4, 400H_2O$) by cadmium is as follows, in gramme-degrees per gramme-molecule:—

Cd, O, SO_3 aq. . . .	89,500
Cu, O, SO_3 aq. . . .	55,960
Difference =	33,540

the difference corresponding to 16,770 gramme-degrees per gramme equivalent, or ·740 volt*. As with normal Daniell cells (§ 114), a small quantity (x) is to be added to this, representing a variable correction dependent on the physical condition of the deposited copper. Evidently the average values above cited (·7475 to ·7525), obtained with bright and electro-cadmium, are sensibly the same as the value $\cdot 740 + x$, thus deduced as representing the net chemical action taking place in the cell; *i. e.*, as with zinc-copper cells, the whole of the

* The value 4410, used in the former parts of these researches for the factor for converting gramme-degrees into C.G.S. units, is employed throughout the present paper; *vide* § 103, footnote.

energy developed in the cell is adjuvant under the conditions obtaining in the above experiments.

In order to compare the results obtained with the amalgamated-cadmium cells with Julius Thomsen's figures, the heat of solution of cadmium (precipitated from the sulphate by zinc, crystalline) in twenty-five times its weight of mercury was determined by means of the calorimeter, 20 grams of cadmium and 500 of pure mercury being employed for each experiment. To insure solution it was found necessary to wash the cadmium with dilute sulphuric acid just before use; otherwise portions remained unwetted and undissolved by the mercury. The final result arrived at as the average of several concordant observations was, that an evolution of heat to the extent of 610 gramme-degrees per gramme-molecule (112 grammes) of cadmium took place during solution. Hence, were cadmium sulphate formed from mercurial solution of metal instead of crystalline precipitated metal, the heat of formation expressed as Cd, O, SO_3 aq. would be $89{,}500-610=88{,}890$ (admitting that Thomsen's value 89,500 applies, without correction, to the metal in the crystalline condition of that experimented with). Consequently the heat of displacement of copper by cadmium from the sulphate is 32,930 per gramme-molecule, or 16,465 per gramme-equivalent, corresponding to ·726 volt. The observed values varied between ·701 and ·717, averaging ·707 with electro-copper and ·706 with amalgamated copper—again not differing from the value deduced from the thermal data by an amount materially outside the limits of experimental errors, especially those due to variation in the heat of formation of salts according as the physical state of the metal employed varies.

It is, however, to be noticed that the above heat of solution of crystalline cadmium in mercury only corresponds to an E.M.F. of ·0135 volt; whilst the average difference in E.M.F. caused by the substitution of fluid amalgamated cadmium for crystalline electro-metal was $\cdot 7525-\cdot 707=\cdot 0455$ volt, a considerably greater amount; so that amalgamating the crystalline metal appears to produce a greater effect on the E.M.F. than corresponds to the heat of solution. Just the same result is produced when cadmium and zinc are opposed (§ 121); on the other hand, the effect on the E.M.F. of amalgamating silver

is sensibly the same as that corresponding to the heat of solution of silver in mercury (§ 129). Probably the difference in the cases of silver and cadmium is due to the oxidizability of the latter by dissolved air, thus rendering the outer surface of the crystalline masses somewhat different from the interior.

Cells containing Cadmium opposed to Zinc.

121. The following table exhibits in brief the results of upwards of thirty series of observations, mostly lasting over several hours, during which period the E.M.F. developed by any given cell remained sensibly steady:—

Variation in E.M.F. due to the use of cadmium and zinc sulphate solutions of different strengths, both solutions being of equal molecular strengths in any given case (strengths varying from $MSO_4 50 H_2O$ to MSO_4 $1250 H_2O$)	Less than $\pm$·0015 volt.

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright cadmium:—				
Fresh electro-metal	−·002	−·007	·005	−·0045
Amalgamated cadmium (liquid)	+·052	+·036	·016	+·044
" " (solid, crystalline)	+·024	+·009	·015	+·0165
Electromotive force of combinations:—				
Amalgamated zinc—Bright cadmium	·367	·361	·006	·364
" " Electro-cadmium	·362	·358	·004	·360
" " Amalgamated cadmium (liquid)	·414	·401	·013	·4075
" " Amalgamated cadmium (solid)	·388	·373	·015	·3805

These figures accord closely with the results deducible from Julius Thomsen's thermochemical data, together with the heat of solution of cadmium in mercury above quoted (§ 120); thus:—

	Free metallic cadmium.	Cadmium dissolved in mercury.
Zn, O, SO_3 aq. . . .	= 106090	106090
Cd, O, SO_3 aq. . . .	= 89500	88890
Difference . .	16590	17200
Difference per gramme-equivalent . . .	8295	8600
Corresponding with volt	·365	·379

The observed electromotive forces* thus do not differ from those corresponding with the thermal values by amounts materially outside the experimental errors. As with the copper-cadmium cells, however, the observed difference in E.M.F. between electro-cadmium (crystalline) and amalgamated cadmium (liquid) is notably greater than that corresponding with the heat of solution of precipitated crystalline cadmium in mercury, being ·407—·3595=·0475 as compared with ·0135 volt.

Volta's Law of Summation of Electromotive Forces.

122. The foregoing experiments clearly show that, as far as cells containing zinc, cadmium, and copper plates are concerned, Volta's law of summation holds, at any rate when the plates are immersed in solutions of their respective sulphates, the solutions being of equal molecular strength; that is, the sum of the electromotive forces generated with a given pair of zinc and cadmium plates, and with that same cadmium plate and a given copper plate, is equal to the E.M.F. generated with the given zinc and copper plates; or, otherwise,

$$\left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cd}\end{matrix}\right. + \left\{\begin{matrix}\mathrm{Cd}\\\mathrm{Cu}\end{matrix}\right. = \left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cu}\end{matrix}\right.$$

where the symbol $\left\{\begin{matrix}\mathrm{Zn}\\\mathrm{Cd}\end{matrix}\right.$ represents the E.M.F. generated with a given kind of zinc plate opposed to a given kind of cadmium plate, each plate being immersed in a solution of its sulphate of constant molecular strength.

* Regnault has shown (*Ann. de Chim. et de Phys.* [3] xliv. p. 453) that the E.M.F. of a cell containing "concentrated" solutions of zinc and cadmium sulphates and plates of these metals was 55, when that of a similar cell with zinc and copper sulphates and plates was 175 (a particuiar thermopile being employed as unit). Taking the E.M.F. of the latter cell as 1·115 volt, that of the former must have been ·350 volt—a value differing from those observed by an amount not outside that possibly due to inequality in the molecular strengths of the two metallic solutions. For, by the use of more dilute cadmium-sulphate solutions (the zinc-sulphate solution remaining the same) an appreciable fall in E.M.F. was found to be produced, the lowest value being 42=·268 volt with solution diluted to $\frac{1}{100}$; on the other hand, decreasing the strength of the zinc-sulphate solution produced far less effect. These and the author's somewhat different results on this point will be discussed in a future paper.

Thus the average results for Daniell cells quoted in Part V. and the above figures give the following comparisons:—

Nature of Plate-surfaces.			Electromotive Force developed.			Normal Daniell.
Zinc.	Cadmium.	Copper.	Zinc-Cadmium.	Cadmium-copper.	Sum.	
Amalgamated.	Electro.	Electro.	·3595	·7525	1·112	Amalgamated zinc and electro-copper 1·111 to 1·116, averaging 1·114.
„	Bright.	„	·364	·7485	1·1125	
„	Amalgamated (fluid).	„	·407	·707	1·114	
„	Amalgamated (solid).	„	·380	·7335	1·1135	
				Mean ...	1·1130	
Amalgamated.	Electro.	Amalgamated.	·3595	·7515	1·111	Amalgamated zinc and amalgamated copper 1·110 to 1·115, averaging 1·113.
„	Bright.	„	·364	·7475	1·1115	
„	Amalgamated (fluid).	„	·407	·706	1·113	
„	Amalgamated (solid).	„	·380	·7325	1·1125	
				Mean ...	1·1120	

A number of direct experiments were also made on this point, using *twin cells* constructed as follows:—Three beakers were arranged containing solutions of copper, cadmium, and zinc sulphates of the same molecular strengths, and plates of electro-copper, bright (or electro-) cadmium, and amalgamated zinc respectively. The copper and cadmium beakers were connected by a siphon tube (with ends covered with bladder) filled with the cadmium sulphate solution; and the cadmium and zinc beakers were similarly connected by a siphon tube containing the zinc sulphate solution. The copper, cadmium, and zinc plates were then connected with cups Nos. 1, 2, and 3 respectively of a switch-board like that represented in fig. 3, Part V. (§ 106); so that by connecting cups 1 and 2 with the electrometer the E.M.F. of the cadmium-copper cell was determined, whilst when cups 2 and 3 were connected the E.M.F. of the cadmium-zinc combination was determined. These readings having been made several times, the zinc and copper plates were transferred to another pair of beakers, containing the same zinc and copper sulphate solutions united by a zinc-sulphate siphon, so as to constitute a

normal Daniell cell after Raoult's pattern, and the E.M.F. of this combination determined. Several pairs of zinc and copper plates were thus used—each pair being read first in the zinc-cadmium-copper combination, then in the normal Daniell cell, and then again in the ternary combination. In each case the difference between the sum of the average electromotive forces of the zinc-cadmium and cadmium-copper couples differed from that of the zinc-copper combination by quantities no greater than the errors of observation of the electrometer-scale (about $\pm 0{\cdot}1$ per cent. when a sufficient number of readings were taken); whilst the average of the small differences observed with different pairs was actually 0, *the small + and − differences due to errors of observation completely balancing one another.* This final result (that no discernible difference was to be found between the sum of zinc-cadmium and cadmium-copper couples, and zinc-copper couples containing the same plates) was obtained in each of several sets of experiments made respectively with solutions of molecular strength $MSO_4\,47\,H_2O$, $MSO_4\,100\,H_2O$, and $MSO_4\,1250\,H_2O$.

Rate of Fall in E.M.F. through so-called Polarization occurring in Zinc-Cadmium and Cadmium-Copper Cells for definite amounts of Increase in the Rates of Current-flow.

123. The experiments made with normal Daniell cells described in Part V. (§§ 103–105) were repeated with zinc-cadmium and with cadmium-copper plates (exposing surfaces of 2·5 and 5·0 square centimetres). The results were similar in character to those obtained with the Daniell cells, no appreciable falling-off in E.M.F. occurring with a current-density of less than some 5 to 10 microampères per square centimetre of plate-surface, but very considerable amounts being observed with stronger currents.

Thus the following table exhibits the values obtained in four experiments—the first three with zinc-cadmium plates, the fourth with cadmium-copper plates, solutions of sulphates of the respective metals employed being used throughout:—

A number of experiments were made with cells containing solutions not of equal molecular strengths (like those described in § 110), for the purpose of finding how much of the diminutions observed with the larger current-densities might possibly be due to the strengthening of the solution round the dissolved plate, and the weakening of the liquid round the other plate, which takes place whilst the cell is in action. The general result was that the maximum possible diminution due to these causes could not exceed about ·04 volt. The details of these experiments and of others allied thereto will be discussed in a future paper.

Effect of varying the Size of one of the Plates, the other remaining constant.

124. The experiments described in Part V. (§ 115) were repeated with various cells containing copper-cadmium or zinc-cadmium plates instead of zinc-copper ones. The following values obtained in four such experiments illustrate the results obtained, indicating that the effect of halving the area of the plate on which metal is deposited is, as with the Daniell cells, greater than the effect of halving the area of the dissolved plate, when the former plate is not mercurialized*. The zinc-cadmium cells, however, differ from ordinary Daniell cells in this respect—that, whilst amalgamating the copper plate of a Daniell cell does not materially alter the relative effect of halving its area, amalgamating the cadmium plate of a zinc-cadmium cell greatly diminishes the relative effect of halving its area, in such sort that, when solid crystalline amalgam is used, the effect of halving the area of the cadmium plates, instead of exceeding, becomes sensibly equal to the effect of halving the area of the zinc plate, whilst when liquid amalgam is used the effect of halving the area of the cadmium plate becomes sensibly less than the effect of halving that of the zinc plate.

* It is noteworthy in this connexion, that when nearly pure wrought-iron plates, immersed in ferrous sulphate solution, replace the zinc plates and zinc sulphate solution of normal Daniell cells, the effect of halving the area of the iron plates sometimes *exceeds* that produced by halving the area of the copper plates

Current, in micro-ampères.	Cadmium-Copper.		Zinc-Cadmium.					
	Bright cadmium—Electro-copper.		Amalgamated zinc—Electro-cadmium.		Amalgamated zinc—Fluid amalgamated cadmium.		Amalgamated zinc—Solid amalgamated cadmium.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Cadmium.	Copper.	Zinc.	Cadmium.	Zinc.	Cadmium.	Zinc.	Cadmium.
1000	·005	·008	·004	·005	·005	·003	·005	·006
2000	·010	·013	·007	·008	·009	·005	·011	·015
5000	·016	·022	·012	·013	·018	·011	·021	·019
10000	·020	·031	·020	·025	·027	·015	·026	·025
20000	·039	·051	·030	·040	·035	·022	·032	·032

II. *Cells containing Silver as one of the Metals, the Salts used being Sulphates.*

125. Three sets of cells, after Raoult's pattern, were constructed, containing respectively zinc-silver, cadmium-silver, and copper-silver couples, the respective plates being immersed in solutions of silver sulphate saturated at ordinary temperatures, and of copper and zinc sulphate of strengths molecularly equal thereto (the silver solution contained 7·25 grammes of Ag_2SO_4 per litre, and had a sp. gr. near to 1·0067); the composition was uniformly $MSO_4\,2360\,H_2O$.

On making series of determinations of the E.M.F.'s of these cells, the following results were arrived at as the effects of varying the nature of the silver surfaces, deduced from the average values of a large number of observations (upwards of 50 sets of comparisons).

Effect of substituting Electro-Silver for Bright Silver.

	Zinc opposed.	Cadmium opposed.	Copper opposed.
Maximum ...	+·017	+·015	+·012
Minimum ...	+·003	+·005	+·004
Range.........	·014	·010	·008
Average......	+·008	+·009	+·0075

Effect of substituting Amalgamated Silver (fluid) for Bright Silver.

Maximum ...	+·102	+·110	+·105
Minimum ...	+·092	+·095	+·092
Range.........	·010	·015	·013
Average......	+·099	+·101	+·1025

Considering the perceptibly wider ranges of variation in these experiments than those usually observed in the zinc-cadmium-copper cells previously described, it is evident that the effect of varying the nature of the silver plate is sensibly independent of the nature of the other metal.

Sometimes, but not invariably, the amalgamated silver plates became solid and crystalline (greenish yellow) on the surface: this result was apparently brought about much more rapidly when the silver was immersed in concentrated zinc sulphate solution than under any other of the conditions obtaining in the various experiments. When this change took place the E.M.F. set up by opposing such a plate to zinc in cells containing sulphates of zinc and silver was always intermediate between that set up in the same fluids by plates of bright silver and of fluid amalgamated silver. The average of several comparisons was as follows:—

Effect of substituting Amalgamated Silver (solid) for Bright Silver.

Maximum............	+·025
Minimum............	+·014
Range	·011
Average	+·021

In this respect silver is analogous to cadmium when the latter is opposed to zinc (§ 121); but the average amounts of increase in E.M.F. due to fluid and solid amalgam are in each instance considerably greater with silver than with cadmium (·099 and ·021 for silver as compared with ·043 and ·016 for cadmium).

On substituting electro- for bright cadmium in the cadmium-silver cells, identically the same average effect was observed as in the cadmium-copper cells (§ 119), viz. an increase in the E.M.F. of from ·002 to ·006, averaging ·004 volt. Similarly, on

substituting amalgamated for electro-copper in the copper-silver cells, practically the same numerical difference in the E.M.F. was brought about as was formerly observed in the zinc-copper cells (§ 107) and the cadmium-copper cells (§ 119), but in the opposite direction, the E.M.F. being *raised* in the copper-silver cells and lowered in the other two kinds: the alteration in the E.M.F. varied between +·005 and −·003, averaging +·0005.

126. On allowing newly set-up cells to stand for several hours, different results were brought about in each of the three cases according as the silver was opposed to zinc, cadmium, or copper. In the first case the E.M.F. invariably fell; the maximum value was observed immediately after the cell was set up, and continued sensibly steady for a variable period of time, a distinct diminution becoming perceptible sometimes after half an hour, sometimes only after two or three hours. With cadmium the value after several hours was somewhat greater than that set up at first and during the subsequent hour or so; and with copper the value attained after several hours was still greater than that exhibited during the first hour or two. The following numbers represent the average alterations thus observed, being the differences between the average readings during the first hour and during a period of from 3 to 5 hours after setting up:—

Bright silver.	Electro-silver.	Amalgamated silver.	Mean.
	Amalgamated Zinc opposed.		
−·010	−·014	−·012	−·012
	Bright Cadmium opposed.		
+·003	+·001	+·003	+·002
	Electro-Cadmium opposed.		
+·003	+·004	+·003	+·003
	Electro-Copper opposed.		
+·009	+·007	+·007	+·008
	Amalgamated Copper opposed.		
+·011	+·009	+·005	+·008

These alterations were traced to the variations in the nature of the surfaces of the plates opposed to the silver; for on

taking out, for instance, an amalgamated zinc plate after 5 hours, and replacing by a freshly amalgamated plate, the E.M.F. was restored to sensibly the same value as at first; and similarly with the other metals. On the other hand, on taking out from two cells, for instance, a zinc and a copper plate after 5 hours, and replacing them respectively in two beakers containing zinc and copper sulphate solutions of the same molecular strengths, and connected by a siphon tube, the E.M.F. of the cell thus formed was found to fall short of the average value of a normal Daniell cell with fresh plates by an amount sensibly equal to the sum of the numerical alterations that had occurred in the zinc-silver and copper-silver cells jointly. It is specially noticeable that, whilst in zinc-copper cells the alteration in the surface of the copper (probably through oxidation) on standing diminishes the E.M.F., in copper-silver cells the alteration is in the opposite direction: with zinc and cadmium the direction of this alteration when opposed to silver is the same as when opposed to copper.

It would seem from all these results that the effect of a given alteration of the surface of one of the plates of a voltaic pair upon the E.M.F. of the pair is independent of the nature of the other plate as regards its numerical value, although the nature of this second plate regulates the direction of the variation in the E.M.F. produced (increase or decrease), and also exerts an influence upon the rate at which the alteration of the plate-surface takes place. Thus it was repeatedly observed that, whereas an amalgamated zinc plate (or an electro-copper one), when forming part of a normal Daniell cell, did not become sensibly oxidized, so as to diminish the E.M.F. of the cell, until after several hours at least had elapsed, a precisely similar plate immersed in the same liquid, but forming part of a zinc-silver cell (or of a copper-silver cell), did become perceptibly oxidized in much less time. In other words, although no measurable current was generated in either case, yet the different amounts of strain (so to speak) set up in the chain of liquid particles between the two plates, according as one was silver or not, did affect the rate of change in the surface of the more oxidizable metal (presumably by varying the rate at which it combined with the oxygen dissolved in the fluid).

127. The following Table exhibits the average results obtained in about 150 sets of observations and comparisons, only those values made during the first hour (or sometimes less) after setting up being taken into account, and all subsequent values being rejected where any diminution through oxidation &c. began to be perceptible *. The values cited as the average effects of substituting for bright silver electro- and amalgamated (liquid) silver are the means of the three sets above quoted (§ 125) obtained respectively with zinc, cadmium, and copper :—

	Maximum.	Minimum.	Range.	Average.
Effect of substituting for bright silver:—				
Electro-silver	+·017	+·003	·014	+·008
Amalgamated silver (liquid)	+·100	+·092	·008	+·101
" " (solid)	+·025	+·014	·011	+·021
Effect of substituting for electro-copper:—				
Amalgamated copper	+·005	—·003	·008	+·001
Effect of substituting for bright cadmium:—				
Electro-cadmium	+·006	+·002	·004	+·004
Electromotive force of combinations:—				
Amalgamated zinc—Bright silver	1·540	1·518	·022	1·528
" " Electro-silver	1·550	1·529	·021	1·536
" " Amalg. silver (liquid)	1·640	1·615	·025	1·627
" " " " (solid).	1·555	1·544	·011	1·549
Bright cadmium—Bright silver	1·173	1·163	·010	1·1675
" " Electro-silver	1·185	1·169	·016	1·1765
" " Amalg. silver (liquid).	1·278	1·261	·017	1·2685
Electro-cadmium—Bright silver	1·176	1·164	·012	1·1715
" " Electro-silver	1·186	1·176	·010	1·1805
" " Amalg. silver (liquid)	1·277	1·267	·010	1·2725
Electro-copper—Bright silver	·422	·411	·011	·416
" " Electro-silver	·429	·411	·018	·4235
" " Amalg. silver (liquid)...	·535	·513	·022	·5185
Amalgamated copper—Bright silver	·420	·411	·009	·4165
" " Electro-silver	·430	·414	·016	.424
" " Amalg. silver (liquid)	·535	·513	·022	·519

Volta's Law of Summation.

128. The values in this table, together with those quoted above for the zinc-cadmium and cadmium-copper cells (§§

* Notwithstanding all care, it is probable that the average results with the zinc-silver cells are too low by a few thousandths of a volt, and those with the other cells slightly too high (vide § 128).

119 and 121) and those given in Part V. for zinc-copper cells (§ 107), clearly prove that Volta's law holds in the case of the sets of combinations

$$\text{(A)} \quad \begin{cases} \text{Zn} \\ \text{Cd} \end{cases} + \begin{cases} \text{Cd} \\ \text{Ag} \end{cases} = \begin{cases} \text{Zn} \\ \text{Ag} \end{cases}$$

$$\text{(B)} \quad \begin{cases} \text{Zn} \\ \text{Cu} \end{cases} + \begin{cases} \text{Cu} \\ \text{Ag} \end{cases} = \begin{cases} \text{Zn} \\ \text{Ag} \end{cases}$$

$$\text{(C)} \quad \begin{cases} \text{Cd} \\ \text{Cu} \end{cases} + \begin{cases} \text{Cu} \\ \text{Ag} \end{cases} = \begin{cases} \text{Cd} \\ \text{Ag} \end{cases}$$

at any rate within the range of possible error due to the somewhat larger ranges of fluctuation in the E.M.F. of silver-containing cells than were observed with zinc-cadmium-copper cells, and to the fact that, although alterations of the oxidizable plates in the cells containing silver was avoided as far as possible by only carrying on the observations for one hour and sometimes less, still it was not practicable wholly to avoid this source of inaccuracy. Thus in the three cases respectively the following figures are obtained (see pages 148, 149).

Average difference in case A	...... =	+·0042
,, ,, ,, B	...... =	+·0027
,, ,, ,, C	...... =	−·0032
Mean	.... =	+·0012

On carrying out twin-cell experiments like those described in § 122, it was found that when a silver plate was placed in the central beaker, and either copper and zinc, copper and cadmium, or zinc and cadmium plates were used in the other beakers, together with solutions of the respective metallic sulphates of the same molecular strengths, the difference between the electromotive forces determined in the twin cell was always sensibly equal to the E.M.F. developed by the pair of plates other than silver employed when taken out and opposed to each other in an ordinary cell containing the same metallic solutions; and this was found to be the case, not only with freshly-prepared plates, but also with plates that had been immersed for hours and had become oxidized on the surface.

Case A.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Zinc.	Cadmium.	Silver.	Zinc-cadmium.	Cadmium-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Bright.	Bright.	·364	1·1675	1·5315	1·528	+·0035
"	Electro.	"	·3595	1·1715	1·531		+·003
"	Bright.	Electro.	·364	1·1765	1·5405	1·536	+·0045
"	Electro.	"	·3595	1·1805	1·540		+·004
"	Bright.	Amalgamated (liquid).	·364	1·2685	1·6325	1·627	+·0055
"	Electro.	"	·3595	1·2725	1·632		+·005
						Average difference =	+·0042

Case B.

Zinc.	Copper.	Silver.	Zinc-copper.	Copper-silver.	Sum.	Zinc-silver.	Difference.
Amalgamated.	Electro.	Bright.	1·114	·416	1·530	1·528	+·002
"	Amalgamated.	"	1·113	·4165	1·5295		+·0015
"	Electro.	Electro.	1·114	·4235	1·5375	1·536	+·0015
"	Amalgamated.	"	1·113	·424	1·537		+·001
"	Electro.	Amalgamated (liquid).	1·114	·5185	1·6325	1·627	+·0055
"	Amalgamated.	"	1·113	·519	1·632		+·005
						Average difference =	+·0027

Case C.

Nature of Plate-surfaces.			Electromotive Forces developed.				
Cadmium.	Copper.	Silver.	Cadmium-copper.	Copper-silver.	Sum.	Cadmium-silver.	Difference.
Bright.	Electro.	Bright.	·7485	·416	1·1645	1·1675	—·003
"	Amalgamated.	"	·7475	·4165	1·164		—·0035
"	Electro.	Electro.	·7485	·4235	1·172	1·1765	—·0045
"	Amalgamated.	"	·7475	·424	1·1715		—·005
"	Electro.	Amalgamated (liquid).	·7485	·5185	1·267	1·2685	—·0015
"	Amalgamated.	"	·7475	·519	1·2665		—·002
Electro.	Electro.	Bright.	·7525	·416	1·1685	1·1715	—·003
"	Amalgamated.	"	·7515	·4165	1·168		—·0035
"	Electro.	Electro.	·7525	·4235	1·176	1·1805	—·0045
"	Amalgamated.	"	·7515	·424	1·1755		—·0050
"	Electro.	Amalgamated (liquid).	·7525	·5185	1·271	1·2725	—·0015
"	Amalgamated.	"	·7515	·519	1·2705		—·0020
						Average difference =	—·0032

For instance, in a pair of experiments with amalgamated zinc, bright silver, and electro-copper plates:—

	Cells newly set up.	After 24 hours.
Zinc-copper in single cell . .	1·115	1·098
Copper-silver in twin cell . .	·417	·425
Sum	1·532	1·523
Zinc-silver in twin cell	1·534	1·522
Difference . .	—·002	+·001

Similar results were obtained in several other experiments of the same kind, both with zinc-copper plates opposed to silver and with the other two pairs (zinc-cadmium and cadmium-copper). The average difference in each case was considerably less than $\pm$·001 volt.

Relations between the Electromotive Forces of Zinc-Silver, Copper-Silver, and Cadmium-Silver Cells and those corresponding to the net Chemical Actions taking place therein.

129. In all the cells hitherto examined in this series of researches there has been shown to be a sensible equality between the electromotive forces generated with clean plate-surfaces of pure metals and those corresponding to the net chemical and physical actions taking place therein. *A priori*, there does not seem to be any evident reason why the same state of things should not exist in the case of cells containing silver plates. Thomsen's thermo-chemical valuations, however, indicate that the electromotive forces corresponding to the three equations

$$Zn + Ag_2SO_4 = 2Ag + ZnSO_4,$$
$$Cd + Ag_2SO_4 = 2Ag + CdSO_4,$$
$$Cu + Ag_4SO_4 = 2Ag + CuSO_4,$$

are considerably higher than those actually observed and tabulated above, thus:—

Zinc-silver.		Cadmium-silver.		Copper-silver.	
Zn, O, SO_3 aq. =	106090	Cd, O, SO_3 aq. =	89500	Cu, O, SO_3 aq. =	55960
Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390	Ag_2, O, SO_3 aq. =	20390
Diff., per gramme-molecule	85700		69110		35570
Diff., per gramme-equivalent.........	42850		34555		17785
Corresponding to volt	1·890		1·524		·784

In each of the three cases the calculated E.M.F. is from ·34 to ·37 volt above the observed values when silver plates not amalgamated are used, indicating that even when only infinitesimal currents are generated a large amount of energy is nonadjuvant. It will be shown in a future paper that this behaviour is more or less marked in other kinds of cells containing silver plates and silver compounds, although the minimum amount of nonadjuvancy observed in any given case is variable with the nature of the saline compounds in the cell*. In order to see whether the rise in E.M.F. produced by amalgamating the silver plate is due simply to the heat of formation of silver sulphate being less when the silver is dissolved in mercury than when it is free, determinations of the heat of solution of silver (precipitated from the nitrate by copper, crystalline) were made. It was found a little difficult to get every trace of silver used dissolved in mercury when 20 grams of silver and 600 of mercury were employed, even though the surface of the former was washed with dilute nitric acid. With smaller amounts of mercury only incomplete solution was effected, more or less pasty amalgam being formed, and a smaller heat-development being then noticeable. The most trustworthy determinations made indicated that during the solution of 108 grams of silver, 2070 gramme-degrees are evolved. Hence the heat of formation of silver sulphate, when the silver is dissolved in mercury, is, per gramme-molecule, $20390-2\times2070=16250$; so that the heat of displacement of silver from silver sulphate, when the metal is ultimately obtained in mercurial solution, is greater than the values above calculated for zinc, cadmium, and copper respectively as precipitating metals by 4140 gramme-degrees per gramme-molecule, or 2070 per gramme-equivalent, corresponding with ·0915 volt. The average difference in E.M.F. between cells otherwise alike, in which crystalline electro-silver and amalgamated silver (fluid) are respectively used, is almost identical with this, being $\cdot101-\cdot008=\cdot093$ volt. As already noticed (§ 120), silver and cadmium differ in their behaviour in this connexion

* Raoult has already obtained numbers (*Ann. Chim. et Phys.* [4] ii. 317 and iv. 392) indicating that the "galvanic heat" (§ 17) of a cell containing copper, silver, and the nitrates of their metals is sensibly below the value due to the net chemical action taking place. These and other similar observations will be discussed in a future paper.

when amalgamated, the inequality between the effect on the E.M.F. actually produced by amalgamation of cadmium and that corresponding with the heat of solution in mercury being probably due to the oxidation of cadmium by dissolved air.

Rate of Fall in Electromotive Force through so-called "Polarization" for definite Amount of Increase in Rate of Current-flow.

130. In order to see whether the electromotive forces of the cells above described are rendered less (when no current passes) than they otherwise would be, through the interfering action of dissolved air or some other similar obscure cause, a number of experiments were made like those described in Part V. (§§ 103–105), with the general result of showing that the deficiency observed in the E.M.F. actually generated when no current passes (as compared with that calculated from the thermo-chemical data) is at any rate not due to any such cause, inasmuch as the E.M.F. generated when a current does pass is always more or less *below* that set up when no current passes, just as with normal Daniell cells and with the zinc-cadmium and cadmium-copper cells above described. Thus, for example, the following numbers were obtained in three experiments, in each of which bright zinc plates were employed, and one in which electro-copper plates were used, the silver plates being in each instance uppermost and surrounded by saturated silver sulphate solution.

Fluid surrounding the zinc plates.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·42.	Zinc-sulphate solution, sp. gr. 1·10.	......
Fluid surrounding the copper plates	......	......	......	Copper sulphate sol., sp. gr. 1·17.
Nature of silver plates.	Electro-silver (crystalline.	Solid crystallized amalgam.	Solid crystallized amalgam.	Electro-silver (crystalline).
Resistance, in ohms, of column of fluid between plates	66·9	76·0	76·6	71·0
Maximum E.M.F.	1·500	1·511	1·546	·401
Current-density, in microampères.	Observed amounts of fall in Electromotive Force.			
20	......	......	·012	·005
50	·016	·010	·021	·011
100	·025	·018	·026	·019
200	·041	·036	·037	·030
400	·062	·067	·059	·047
600	·080	·097	·077	·063
1000	·116	·144	·112	
2000	·209	·258	·177	

These numbers are represented graphically by the curves marked respectively V., VI., VII., and VIII. in the figure.

On trying experiments, like those described in § 110, to see how far the falling-off in E.M.F. when a current is generated could be due to the accumulation of zinc (cadmium or copper) sulphate round the plate opposed to the silver, it was found that the maximum possible effect due to this cause could not exceed about ·04 volt with zinc and cadmium, and ·02 with copper. These experiments, and others of a similar nature, will be discussed in a future paper.

On comparing the eight curves represented in the figure with those previously described as obtained with various forms of Daniell cell (Part V. § 105), it is noticeable, first, that the curves obtained with the zinc-cadmium cells underlie all the others (I., II., and III.); secondly, that the curve with the cadmium-copper cell (IV.) is practically identical with one of the Daniell-cell curves—indicating consequently that, whilst the substitution of copper for cadmium in a zinc-cadmium cell raises the position of the curve (*i. e.* increases the rate of fall in E.M.F. according as the current-density increases), the substitution of cadmium for zinc in a Daniell cell does not materially alter the position of the curve; thirdly, the curves with the zinc-silver and copper-silver cells overlie all the others, whilst the copper-silver curve (VIII.) is not widely different from the zinc-silver curves (V., VI., and VII.)—indicating that, whilst the substitution of silver for copper in a Daniell cell largely raises the position of the curve, the effect of substituting copper for zinc in a zinc-silver cell is very much less marked. In other words, *the nature of the dissolved metal affects the rate of decrease in E.M.F. with increasing current-density much less than does the nature of the deposited metal; whilst the less the heat of formation of the salt of the latter that is decomposed by the passage of the current, the more rapid appears to be the rate of fall in the E.M.F. of the cell as the current-density increases.* As regards the first part of this general conclusion, it is precisely what also results from the majority of the previously described experiments on the effect of halving the area of the dissolved plate, as compared with that produced by halving the area of the plate on which metal is deposited. The following experiments with cells containing silver plates also give the same general results.

Effect of Varying the Size of one of the Plates in Cells containing Silver as one of the Metals, the other Plate remaining unaltered.

131. By operating in the way described in § 115, the following results were obtained in four sets of observations with zinc-silver and copper-silver cells, showing that, in all cases, halving the area of the silver plate produces a sensibly greater decrease in the E.M.F. set up with a constant rate of current-flow than is effected by halving the area of the plate opposed to the silver.

Current-density, in micro-ampères.	Zinc-Silver.						Copper-Silver.	
	Amalgamated zinc—electro-silver.		Amalgamated zinc—crystalline amalgamated silver.		Amalgamated zinc—crystalline amalgamated silver.		Electro-copper—electro-silver.	
	Effect of halving area of		Effect of halving area of		Effect of halving area of		Effect of halving area of	
	Zinc.	Silver.	Zinc.	Silver.	Zinc.	Silver.	Copper.	Silver.
1000	·009	·011	·007	·026	·008	·019	·004	·011
2000	·012	·024	·009	·045	·012	·036	·010	·023
3000	·014	·038	·011	·059	·015	·047	·017	·028
5000	·016	·064	·020	·103	·021	·070		
8000	·028	·093	·038	·186	·029	·122		
10000	......	......	......	......	·034	·151		

Summary of Results.

132. The foregoing results may be thus summarized :—

Cells containing zinc and cadmium or cadmium and copper plates, immersed in solutions of the sulphates of these metals respectively, are closely analogous to ordinary Daniell cells (containing zinc sulphate solution). Slight variations in the E.M.F. generated are introduced by varying the condition of the plate-surfaces; but in all cases the maximum E.M.F. actually generated with clean pure plate-surfaces and with solutions of equal molecular strengths is close to that calculable from the net chemical action taking place in the cell when generating a current. When the cadmium plates are not amalgamated, or are covered with crystalline solid amalgam, the electromotive forces are close to ·75 and ·36 volt for cadmium-copper and zinc-cadmium cells respectively, the values corresponding to the net chemical actions as deduced

from Thomsen's thermo-chemical results being substantially the same. When the cadmium plates are covered with fluid amalgam, the electromotive forces are lower in the first case and higher in the second by upwards of ·04 volt—a quantity distinctly exceeding in magnitude the E.M.F. corresponding with the heat of solution of cadmium in mercury, although of the same sign.

(2) The electromotive forces of zinc-silver, cadmium-silver, and copper-silver cells containing the respective sulphates of these metals differ from those of zinc-cadmium, zinc-copper (Daniell), and cadmium-copper cells in this respect, that the maximum electromotive forces generated (the fluids being of equal molecular strength) are not sensibly the same as those calculated from Julius Thomsen's thermal data, but in every case fall short by an amount not far from ·35 volt. When the silver plates are not mercurialized, or are coated with crystalline amalgam, the electromotive forces (which vary slightly with the precise nature of the plate-surfaces) are, in the three cases, near to 1·53, 1·17, and 0·42 volt respectively, the metallic solutions being of equal molecular strength. When the silver plates are covered with fluid amalgam, the electromotive forces are in each case about ·09 volt higher than the values obtained with electro-deposited crystalline metal, this increase almost exactly coinciding with the increment corresponding with the heat of solution of silver in mercury.

(3) As long as the cadmium and zinc [or copper] solutions employed are of the same molecular strength within the limits indicated respectively by $MSO_4\,50H_2O$ and $MSO_4\,1250H_2O$, the E.M.F. developed with a given pair of cadmium and zinc [or copper] plates is sensibly independent of the actual strength of the solutions, these cells behaving precisely like Daniell cells in this respect. With Daniell cells the solutions are practically of the same molecular strength when they are of the same specific gravity; but with the other cells containing cadmium this is not so, cadmium-sulphate solution being uniformly more dense than either zinc or copper solution of the same molecular strength.

(4) The effect on the E.M.F. of a cell of a given alteration in the nature of the surface of either a zinc, copper, cadmium, or silver plate is sensibly the same numerically whichever

other one of these four metals be opposed to it; but the direction of the alteration is opposite according as the plate is the anode or the kathode of the combination.

(5) Volta's "Law of Summation" universally holds within the limits of experimental error in all the cases examined; that is, the electromotive forces of zinc-cadmium, cadmium-copper, and copper-silver combinations are such that, for any given kinds of plate-surfaces, the sums of the two first, of the two last, and of the three together are respectively equal to the electromotive forces of zinc-copper, cadmium-silver, and zinc-silver combinations.

(6) Zinc, copper, and cadmium plates alter superficially (probably in consequence of oxidation by dissolved air) more rapidly when opposed to silver than when opposed to any other one of these four metals, on being immersed in solutions of their respective sulphates forming one half of a cell on Daniell's principle—no current being generated by the cell, the measurements being made by means of a quadrant electrometer.

(7) With all the cells examined the behaviour when generating a current is analogous to that of a normal Daniell cell: when the current-density exceeds a few microampères per square centimetre of plate-surface, a more or less marked diminution in the E.M.F. ensues, the falling-off being the greater the greater the current-density. With moderately strong currents the diminution far exceeds the maximum possible amount due to accumulation of dissolved salt round the plate dissolved, and exhaustion of solution round the other plate. *Cœteris paribus,* the rate of fall in E.M.F. as the current-density increases is the more rapid the lower the heat of formation of the metallic salt decomposed in the cell so as to deposit the metal, and is comparatively but little affected by the nature of the dissolved metal.

(8) The effect of halving the area of the plate on which metal is deposited is usually to cause a greater diminution in the E.M.F. than is produced by halving the area of the dissolved plate; amalgamated cadmium plates in zinc-cadmium cells, however, form an exception to this rule.

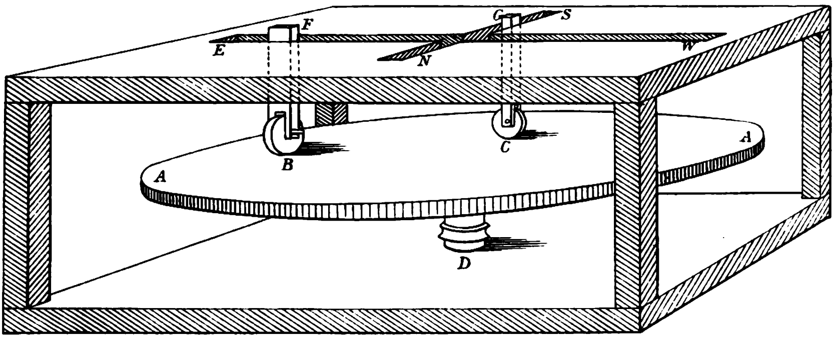


Fig: 1.

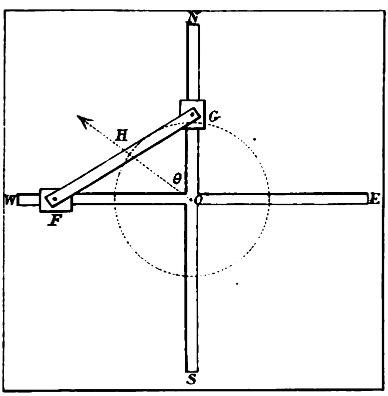


Fig: 2.

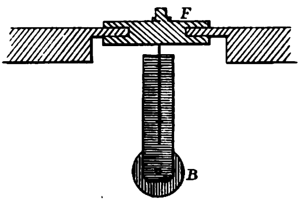


Fig: 3.

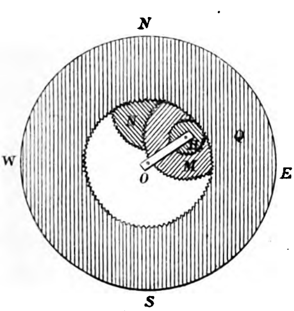


Fig: 4.

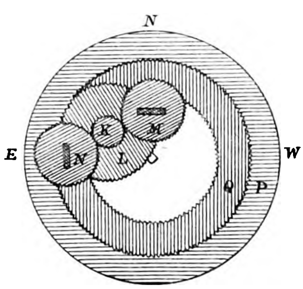


Fig: 5.

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XVI. *An Integrating Anemometer.* *By* WALTER BAILY*.

[Plate VII.]

THE object of the instrument described in this paper is to resolve the velocity of the wind in two directions at right angles to one another, and to obtain the time-integral of each part separately.

The instrument contains a horizontal plane, in which are two slits N S and E W, forming a cross to be placed with its arms towards the cardinal points. In these slits are sliders F, G, connected by a bar of constant length. O is the centre of the cross, H the centre of the bar. The locus of H is a circle with centre O. A weathercock or some equivalent mechanism is to keep H in such a position that the radius O H is in the direction of the wind. The sliders carry beneath them wheels, B, C, whose planes are perpendicular to their respective slits, and whose centres are beneath the pivots joining the slits to the bar. [See figs. 1, 2, 3, Plate VII. Fig. 1 gives a perspective view of the instrument, omitting some points; fig. 2 gives a view of the top of the instrument; and fig. 3 gives a section of a slit and slider, and shows the wheel carried by the slider.] The wheels B, C rest on a disk, A (fig. 1), which revolves about a vertical axis immediately below O. The disk A is to be rotated by Robinson's cups, or some equivalent mechanism, so as to have a velocity proportional to that of the wind. The pieces which carry the wheels B, C should be allowed some play in a vertical direction; and the contact of B and C with A can then be maintained either by their own weight or by the use of a spring. The number of rotations of B in a given time is proportional to the time-integral of the resolved part of the wind in one direction (say, north); and the number of rotations of C is proportional to the time-integral of the resolved part of the wind in a direction at right angles to the first (say, west).

Let Ω be the angular velocity of the disk A; ω, ω' the angular velocities of the wheels B, C; m, m' the number of their rotations in a given time t; b their radius, a the length of the bar; θ the angle between the direction of the wind and

* Read June 10, 1882.

(say) the north; then $b\omega = a\sin\theta\,.\,\Omega$, and $b\omega' = a\cos\theta\,.\,\Omega$; and the integrals required are

$$\int_0^t \Omega\sin\theta\,dt = \int_0^t \frac{b}{a}\omega\,dt = \frac{b}{a}\pi m$$

and

$$\int_0^t \Omega\cos\theta\,dt = \int_0^t \frac{b}{a}\omega' dt = \frac{b}{a}\pi m'.$$

Therefore m, m' are proportional to the required integrals.

Each slider might carry a train of wheels to record the number of rotations; or an electrical arrangement might be made in which each wheel should complete a circuit at each rotation and the number of contacts should be recorded. In the latter case, as no distinction is preserved as to the direction in which the wheels revolve, it becomes necessary to have four circuits, one for each cardinal point, with a recorder in each, and to have one connected with each arm of the cross.

A working model of the instrument above described was exhibited at the Meeting, and was fitted with an electrical arrangement such as I have mentioned.

I have since discovered that the slits, sliders, and bar above described may be replaced by a train of cogged wheels. (Fig. 4 represents the upper, and fig. 5 the under surface of the train.) A bar turns in a horizontal plane about O, and is kept in the direction of the wind. This bar carries three wheels, H, K, L, having the same axis. The length of the bar from one pivot to the other is supposed to be an inch and a half. The wheels H, K are rigidly connected; and L lies between them and turns independently. H and K are 1 inch, and L is 3 inches in diameter. L rolls on the inner edge of P, and H rolls on the inner edge of Q, the diameters of P and Q being 6 and 4 inches respectively. Two wheels, M and N, whose diameters are 2 inches, are carried by the wheel L, and have their centres at the extremities of a diameter of L. M and N are in the same plane as K, and are therefore touched by it. As the bar rotates, M and N move without rotation, and their centres move in straight lines passing through O at right angles to one another, and are at a fixed distance apart, and have the line joining them bisected by the bar, which is the direction of the wind. Hence M and N may be used to carry the wheels B, C (fig. 1) instead of their being carried by the sliders F and G.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

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XVII. *On Central Forces and the Conservation of Energy.*

By WALTER R. BROWNE, *M.A., M. Inst. C.E., late Fellow of Trinity College, Cambridge**.

IT is well known that the ordinary proof of the principle known as the Conservation of Energy assumes the forces acting to be Central Forces†; but the intimate connexion existing between these two facts—the existence of Central Forces and the Conservation of Energy—has not, so far as I am aware, been thoroughly examined. I shall here attempt to show that the two necessarily imply each other; so that not only is the Conservation of Energy true if the system is a system of central forces, but the Conservation of Energy is not true if the system is any thing but a system of central forces.

For the sake of simplicity I will confine myself to the case of two particles, and suppose them so far apart, in proportion to their dimensions, that each may be treated as if concentrated at its centre of gravity. Let the particles be A and B, and consider the motion of B with reference to A as fixed. Suppose B to be moving away from A, and to be acted upon by a moving force due to the action of A. Let it move from a dis-

* Read November 11, 1882.

† This is recognized explicitly by Clausius, 'Mechanical Theory of Heat,' p. 16.

tance a to a distance $a+b$, and let F be the resolved part of the moving force in the line AB. Then the energy exerted by B during this motion in overcoming the attraction of A is represented by

$$\int_a^{a+b} \mathrm{F}\,dx.$$

Let v_1 be B's initial velocity, m its mass. Then at the end of the motion v_1 will be reduced to v, where v is given by the equation

$$\frac{m}{2}(v_1^2-v^2)=\int_a^{a+b} \mathrm{F}\,dx.$$

Let us suppose b to be such that $v=0$, so that

$$\frac{m}{2}v_1^2=\int_a^{a+b} \mathrm{F}\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Then, when B arrives at distance $\overline{a+b}$, its velocity, and therefore its kinetic energy or *vis viva*, will be reduced to zero. There is therefore a loss of energy, so far as B is concerned. But now let us suppose that B is left free to return towards A, and that it passes back again over the space b. Then, if F continues to act, A will exert during the motion an amount of energy on B, or will do an amount of work upon B, which will be represented by

$$\int_{a+b}^{a} \mathrm{F}\,dx;$$

and when B has reached the distance a, it will have gained a velocity V, given by the equation

$$\frac{m}{2}\mathrm{V}^2=\int_{a+b}^{a} \mathrm{F}\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now if $\mathrm{V}=-v_1$, then $\mathrm{V}^2=v_1^2$: hence we shall have the two particles in the same position as at first, and the kinetic energy of B will be the same as at first. Therefore there will have been no loss or gain of energy on the whole; and the energy is then said to have been *conserved* during the motion. At the time when B's velocity is zero, the energy of the system is represented by the potential energy of A—that is, the power A has of subsequently doing the work $\int_{a+b}^{a} \mathrm{F}\,dx$ upon B

At other times during the motion, the energy of the system is partly potential energy of A, partly kinetic energy of B.

We thus see that it is essential to the Conservation of Energy that V^2 should $= v_1^2$. But by equations (1) and (2) this is equivalent to the equation

$$\int_a^{a+b} \mathrm{F}\, dx = \int_{a+b}^{a} \mathrm{F}\, dx; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

these two expressions representing the two amounts of energy exerted, as described above.

It is therefore essential for the conservation of energy that F should be a function of a form such that equation (3) may hold. We have now to inquire what this form must be, or, in other words, within what limits F may be allowed to vary so that the equation (3) may still hold.

Now we have not supposed the constitution of A and B, or their relations to each other, to vary in any way except in regard to space and in regard to time; and we have every reason to believe that these are the only variations which take place in the ultimate molecules of matter. Hence we need only consider variations with regard to space and time.

Now if F be any function of time, then, since some time must have elapsed between the exertion of the two amounts of energy represented by the two sides of equation (3), it follows that for every value of F in the right-hand expression the time will be greater than for the corresponding value of F in the left-hand expression; and therefore the sums of the two sets of values, or the two integrals, cannot be equal. Hence F cannot be a function of time.

We have therefore only to consider variations in space. Now, if we confine our attention to one plane, we know that any variation of B's place in that plane may be represented by a change in the values of x and θ; where x is B's distance from A, and θ the angle which the axis of x makes with some fixed line in the plane*. Then it is easy to show that F must not vary with θ. For if it does, let us suppose that when B has come to rest, and before it is allowed to return, it is made to rotate about A through an angle $d\theta$, and again brought to

* We here make no assumption except that the force varies as B's position in the plane varies; which is essential to every theory on the subject.

rest. Then the circumstances of A and B are unchanged; for the kinetic energy given to B during the rotation has been taken out again in stopping it. But if B is now allowed to return towards A, then, for every value of F in the right-hand expression, the value of θ will be greater or less by $d\theta$, the amount of the change, than for the corresponding value in the left-hand expression; and therefore, as before, the two integrals cannot be equal. Similarly, if we take coordinates x, θ, ϕ, in three dimensions, it will follow that F cannot be a function of ϕ.

Hence we are left with the conclusion that F can only be a function of r; in other words, the force with which A acts upon B always tends towards A, and varies, if it varies at all, according to the distance from A only. But this is the definition of a central force.

[The proof just given, that F cannot vary with θ, appears quite general. But it is easy to show that any particular law of force which can be imagined, other than that of a central force, is inconsistent with the conservation of energy. Thus, suppose the force to vary according to B's distance from some other point in the plane than A; then that distance can always be expressed in terms of the coordinates of its extremities, and therefore in an expression involving θ, which is inadmissible. Again, suppose the force to vary according to the perpendicular distance of B from some line in the plane. Then, if B move parallel to that line the force is constant, while if it be perpendicular it varies from zero; and it is easy to see that if B moves perpendicular to that line, and if, before it is allowed to return, it is rotated till the line AB is parallel to that line, then the two integrals will not be equal. Again, suppose the force to act upon a certain line only, so that when B is off that line no force acts upon it; then, if we suppose the return journey made parallel to that line, the energy on that journey is zero.]

We have throughout taken F as the force between A and B, resolved along the line joining them. We have still to consider the possibility of there being another component always at right angles to this line. This component, if it exists, will produce a rotation of B round A, which will increase B's kinetic energy; and as there will be nothing to balance it, this

increase will go on for ever; so that the conservation of energy would not be true in this case.

I have thus proved, I believe, the proposition with which I started—namely, that the doctrine of central forces and that of the conservation of energy are mutually interdependent, so that one is not true without the other. In general, as remarked at the beginning, the existence of central forces is assumed, and the conservation of energy deduced from it. But ne process may be reversed. The conservation of energy may be considered to rest, as a general law of nature, on the broad basis of observed facts, such as the conversion of heat, electricity, chemical actions, &c. into mechanical work, and the reconversion of mechanical work into these other forms of energy. There can be no doubt that the evidence of this character is of very great weight; and I am myself disposed to accept it as conclusive. But it must be pointed out that, unless the above investigation be false, it involves our accepting a mechanical definition of matter substantially to the following effect*:—"Matter consists of a collection of centres of force, acting upon each other according to laws which do not vary with time but do vary with distance."

This conception of matter is of course an old one, being that usually known by the name of Boscovitch. It has not, however, been generally accepted by writers on Mechanics; and in recent times certain special objections have been raised against it, which it seems well briefly to consider.

1. An objection, due to Professor Maxwell†, is that the conception does not comprise the idea of inertia, which is a fundamental fact with regard to matter. But when we say that a body has inertia, we simply mean that a finite force, acting upon it for a finite time, generates only a finite velocity. Hence it follows that any body we can see or feel, or know and investigate in any way, must have inertia; for suppose a body to possess no inertia, then the first time any force was applied to it, it would at once be removed to an indefinite distance, and would therefore be beyond the reach of investigation. To

* This definition has been already given in a pamphlet entitled 'The Foundations of Mechanics' (Charles Griffin and Co., 1882).

† 'Theory of Heat,' p. 85.

say that matter has inertia is therefore merely to assert the general principle that any thing our senses can deal with must be finite; and it is therefore a condition anterior to any theory of matter, not a part of such theory.

2. An objection given by Lamé* is that bodies, and especially homogeneous crystals, are not, within the limits of observation, denser at the centre than they are at the surface, which on the theory of central forces they apparently should be. But on this it may be observed as follows.

It may be admitted that collections of centres of force, *at rest under their mutual actions*, would be more dense towards the centre. We know no such bodies in nature. The nearest approach to it is the case of bodies so large that their molecular motions, and also their want of homogeneity, may be neglected in comparison of their mass. But the condition of large bodies does appear to agree with the theory; *e. g.* even the *mean* specific gravity of the earth (5·6) is greater than that of all bodies, except a few metals, at the surface. The want of homogeneity can have little influence at the temperature and pressure which prevail in the interior.

Again, it is known that, in all bodies, the actual centres of force must be bound up together in molecules so closely as to form coherent wholes, which no known force can change or break up. The relations in a crystal therefore are not those among separate centres of force, but among separate molecules.

Again, these molecules, being hot, are in rapid and continuous motion.

Lastly, the laws of the forces of cohesion, whether in the interior of a molecule or between one molecule and another, are unknown.

In such circumstances, can it be held impossible that there should be laws of distribution of force such that in small bodies like crystals the difference in density at the centre and surface should be insensible? Lamé does not attempt to give any rigid proof that the uniform density of crystals (even if accurately true) is really incompatible with the theory of central forces. It is therefore merely a presumption, and a presumption which seems seriously weakened by the fore-

* *Elasticité des Corps solides*, p. 333.

going considerations; it cannot therefore be allowed to have any weight as against actual evidence.

3. An objection, due to Prof. Tait, is that we have no right to assume that force has any objective existence at all, or is any thing more than the rate of change of motion—and that in fact it cannot have an objective existence, because it can be affected with a positive or negative sign. But, with regard to the first part of this objection, a force is defined in Mechanics simply as a cause of motion; and therefore the remark is a mere denial of the general principle of causation. This is not the place to discuss the truth of that principle; but it may be observed that it is perhaps almost the only principle which may claim to have been accepted by all thinkers of all schools and in all ages. With regard to the second part of the objection, the circumstance that a force, or rather the symbol of a force, may be affected, for purposes of calculation, with a + or − sign is simply due to the fact that a force has a definite sense, or direction; and that direction is one of the properties of things to which the conception of positive and negative may properly be applied. For the same reason lines may be represented as + or −, as in algebraical geometry; but they are not therefore regarded as non-existent. Nor is direction the only fact to which the conception applies; *e. g.* in treatises on algebra it is often pointed out that capital may be taken as positive and debt as negative. Will it therefore be argued that money has no real existence?

4. In some quarters an objection appears to be felt to the theory of central forces, on the ground that it involves the conception of action at a distance, which is supposed to be "unthinkable." I am not aware that the term "unthinkable," which is a new one, has ever been defined. Until it has been, it is impossible to say whether action at a distance is unthinkable, or whether the fact of a conception being unthinkable is sufficient reason, or any reason, for holding it to be untrue.

It seems desirable, before leaving the subject, to say a few words upon a theory which has been set up as a rival to that of central forces, and in some quarters has met with considerable favour. This theory supposes that bodies can act on each other only when in absolute contact; and that all the pheno-

mena of the universe may be accounted for by the knockings together of a number of ultimate atoms, considered as very small impenetrable bodies, moving with high velocities in space.

It might be urged that before such a theory can be seriously discussed, it must be shown capable of explaining (as the theory of central forces certainly does explain) the facts and principles of Mechanics. I am not aware that this has been done. I may, however, point out that the theory is not inconsistent with the conservation of energy; that is to say, it can be reconciled with it by certain special assumptions. For the proof of that principle, as given above, does not necessarily imply that the forces acting are *continuous*. If the attraction of A be supposed to act on B by equal impulses at certain intervals of space, or distances from A, which distances remain always the same, then the proof will still hold; for B will be acted upon by exactly the same number of impulses, and at exactly the same places, on its return journey as on its outward journey, and the effects will therefore be the same. Now the "collision" theory above mentioned may be taken to represent the extremest possible case of this discontinuous action—there being then but one impulse, and that acting when A and B are in absolute contact.

Let us, however, consider the assumptions involved, if the conservation of energy is to hold in this extreme case. Imagine two "ultimate atoms," of equal mass, to meet each other with equal velocities in the same straight line. This is clearly a possible case under the theory; and the conservation of energy must therefore be consistent with it. Then the instant before the atoms meet they have no action upon each other, and the instant after, by symmetry, they must either be at rest or must have passed through one another. As the latter is contrary to the hypothesis, they must be at rest. Hence a finite mass moving with a finite velocity has been brought to rest in a space infinitely small; and therefore the impulse acting upon it must have been strictly infinite in amount. This collision therefore (and it is easily seen that the same will be true of all collisions) occasions the instantaneous development of a strictly infinite force. The atoms being brought to rest, there is no reason to be given why any thing further should happen.

But we must assume it as an axiom that a further mutual impulse is then given, sufficient (if the bodies are supposed perfectly elastic) to cause each to return on its path with a velocity exactly equal to that with which it arrived. This further impulse must also be instantaneous and infinite; for, force being the cause of motion, if the impulse were finite it would at once cause the bodies to separate through an indefinitely small space, and then, *ex hyp.*, no further action could take place, and the bodies would recede from each other with indefinitely small velocities. If, then, we make these three assumptions—(1) that there is an infinite impulse developed on the collision, which brings the atoms to rest, (2) that there is a further infinite impulse, which separates them, (3) that this further impulse, while infinite, is such as exactly to reverse the previous motion of each particle—then the conservation of energy may still be supposed to hold through the collision.

It remains to ask whether there are any advantages in the collision theory such as would warrant us in discarding the principle of continuity, and in making the somewhat violent assumptions described above. The advantages specially claimed by its advocates appear to be that it does away with the conception of action at a distance, and also with that of potential energy. The latter supposition, however, is not correct. At the instant when the two atoms are at rest their actual energy is zero, and the energy existing is entirely potential, being due to their capacity of generating a return velocity equal to that of arrival. Of the former supposition I have already spoken; and I may add that I have elsewhere* shown it to be impossible to explain certain elementary facts of physics without the hypothesis of action at a distance.

XVIII. *The Electrical Resistance of Selenium Cells.* *By* Shelford Bidwell, *M.A., LL.B.*†

In June 1881 a paper was read before the Physical Society by Dr. Moser, on "the Microphonic Action of Selenium Cells." In this paper a very ingenious attempt was made to

* "On Action at a Distance," Phys. Soc. 1881; Phil. Mag. Dec. 1880.

† Read November 25, 1882.

show that the effect of light in reducing the electrical resistance of selenium might be accounted for on perfectly well understood principles, without assuming the existence in the case of this substance of some law or property *sui generis* and hitherto unobserved.

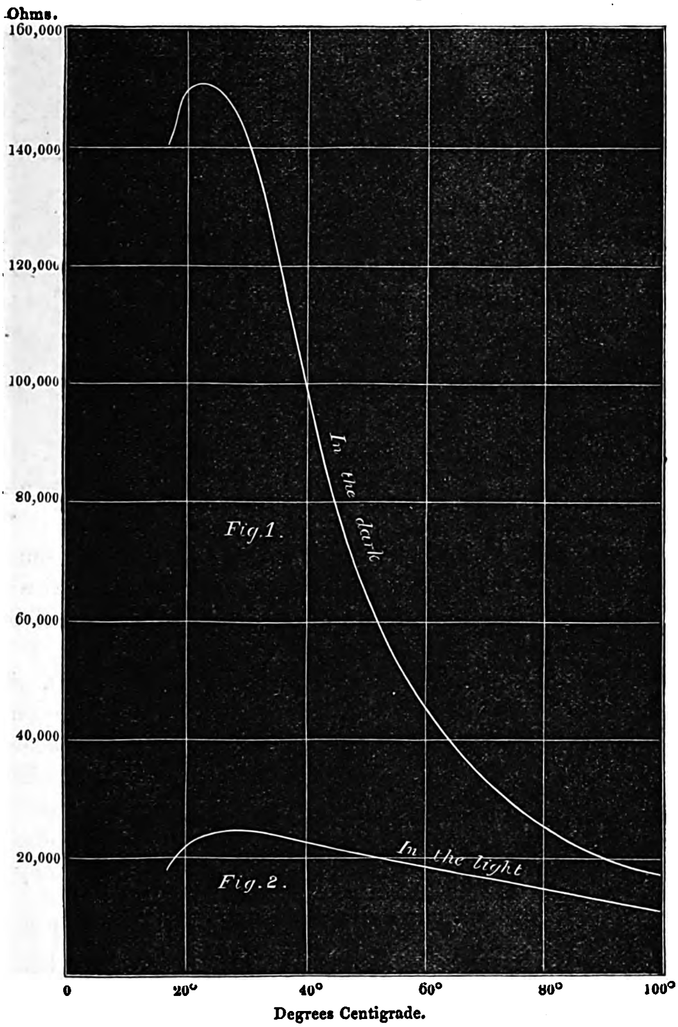
Dr. Moser's theory is shortly as follows:—There is always imperfect contact between the metallic electrodes and the selenium which together constitute a so-called "selenium cell." Selenium reflects the invisible portions of the spectrum, absorbing principally the visible or illuminating rays: the vibrations thus taken up assume the form of heat; and the temperature of the selenium cell is thereby raised*. In consequence of this rise of temperature the selenium expands; it is accordingly pressed into closer contact with the electrodes, and, as in the case of the microphone, the resistance of the system is proportionately diminished. When the cell is screened from the light, the absorbed heat is quickly radiated away; the selenium contracts to its former volume, and the original degree of resistance is restored. Thus, according to Dr. Moser's paper, the whole mystery is easily and completely explained.

This theory can evidently be submitted to a very simple and conclusive test. If it is true that the observed effects are due merely to a rise of temperature, then it is clearly immaterial whether such rise of temperature is brought about by the heating action of light or by the direct application of heat in the ordinary way. Instead of exposing a selenium cell to the light, let it be enclosed in a dark box and warmed over a gas-burner; then, if the theory be correct, the resistance of the cell should at once begin to fall. This, however, is not found to be the case. I have in my possession a number of selenium cells the resistance of which is immediately diminished by the smallest accession of light; but in the case of all of them (except one, of which I shall say more presently) the immediate effect of the direct application of heat is not a a fall, but a rise in the resistance. When the temperature of the cell reaches a point which is in general a few degrees higher than the average temperature of the air a maximum

* "*Selenium,*" Dr. Moser says, "*is heated by light.*"

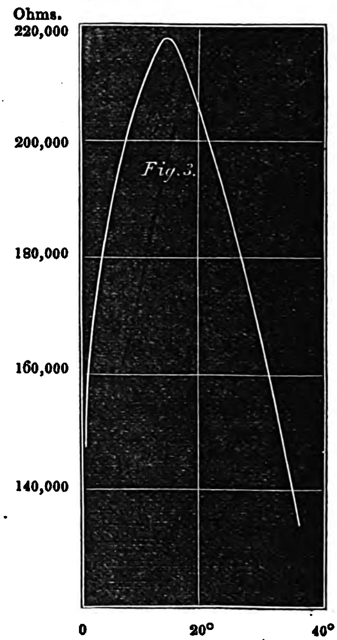
resistance is attained; and if the heating is continued, the resistance begins to decrease.

I gave a short account of this phenomenon in the 'Philosophical Magazine' of April 1881. Since this was published, I have made further and very careful experiments, the results of which are shown in the curves, figs. 1, 2, and 3.



A selenium cell was placed in an air-bath in absolute dark-

ness, the bulb of a thermometer being very near its surface. The temperature of the air was 17° C., and the resistance of the cell at the beginning of the experiment was 140,000 ohms. The bath was then very slowly heated, and the resistance measured at every degree. At first the rise was very rapid (see fig. 1); then more gradual until the temperature reached 23°, when the maximum resistance of 150,000 ohms was attained. With continued heating the resistance fell, slowly at first, then more rapidly, then again slowly (as shown by the curve), the final measurement at 100° being only 16,000 ohms*.



The same cell was afterwards submitted to the combined action of heat and light. A glass beaker was fitted with a wooden cover, to which the selenium cell was attached so as to hang perpendicularly inside the beaker; the beaker was placed in a sand-bath which was heated by a Bunsen burner, and the cell was illuminated by a powerful paraffin-lamp at a distance of 30 centimetres.

At 18° its resistance was only 19,000 ohms (see fig. 2). As in the former case, the first application of heat was accompanied by a rise, though smaller and more gradual than before, the maximum of 24,000 ohms occurring at about 29°. The fall which followed was also very gradual, the resistance at 100° being 10,000 ohms, or only 14,000 less than the maximum, as against a difference of 140,000 in the former experiment.

In these experiments it might possibly be suspected that the initial small rise of resistance is due to some accidental

* When the cell was removed from the air-bath, its resistance in the dark in air at 18° was found to have increased to 90,000 ohms.

disturbing cause, and does not point to any essential characteristic of selenium, or rather perhaps of selenium cells. The following experiment seems, however, to settle the point conclusively. One of my selenium cells (the exceptional one above referred to) did not at ordinary temperatures exhibit this peculiarity. When heated, its resistance at once went down without any preliminary rise. This cell was placed in air at a temperature of 0°, and after remaining for half an hour its resistance was found to be 147,000 ohms. The temperature was then slowly raised; and, as I expected, the resistance at first went up, attaining a maximum of 219,000 ohms at 13°, after which it went down to 134,000 ohms at 36°, when the experiment was stopped. The curve fig. 2, which is on the same scale as the others, shows the results in a very striking manner, altogether excluding the possibility of accidental disturbance. This particular cell differed from others only in the fact that it acquired its maximum resistance at a temperature slightly below instead of slightly above the average temperature of the air.

The supposition that light produces its effect by heating is further negatived by the fact, that a comparatively high degree of temperature is required to bring down the resistance of the cell to the point to which it is instantly reduced by exposure to a strong light. When a selenium cell is for a moment exposed to sunlight, it does not become perceptibly warm to the touch; but the amount of dark heat necessary to effect the same reduction in its resistance as is caused by a moment's sunshine would certainly render it too hot to handle.

Again, those who have experimented with the photophone know well that the best results are obtained only when precautions are taken to exclude those rays which are especially instrumental in producing heat, as by filtering the beam of light through a solution of alum. Dark radiation does indeed *per se* diminish the resistance of selenium; but the diminution due to dark radiation is to some extent masked by the rise of temperature which accompanies it, and which generally tends to produce the opposite effect.

To me it seems clear that the electrical effects of radiation are, in this case at least, no more due to the intermediate

action of temperature than are the chemical effects which radiation sometimes produces, as in the various photographic processes. All such effects are no doubt ultimately of a mechanical nature; but while increased temperature may result from vibrations the periodicity of which varies between very wide limits, the other effects arise only when there is some more or less definite relation between the period of the æther-waves and the molecular constitution of the substance upon which they act.

In its peculiar sensitiveness to the visible part of the spectrum selenium seems, so far as our present knowledge goes, to stand almost, if not quite, alone*.

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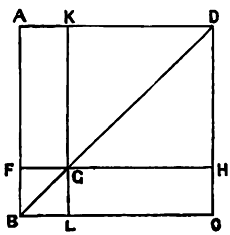
XIX. *On the Graphic Representation of the Law of Efficiency of an Electric Motor. By* Professor SILVANUS P. THOMPSON.

(1) VARIOUS graphic constructions have been given at different times to represent the work performed by an electric motor and the electric energy expended upon it. The main defect of those hitherto given has been that they present these quantities in such a manner that a comparison of the two, which would show the efficiency of working of the motor, is not immediately evident. Moreover it has not been possible hitherto to show on one construction both the law of maximum rate of working and the law of efficiency. The following construction makes them evident to the eye.

Fig. 1.



Let the vertical line A B (fig. 1) represent the electromotive force, E, of the electric supply when the motor is at rest. On A B construct a square A B C D, of which the diagonal B D may be drawn. Now measure out from the point B, along the line B A, the counter electromotive force of the motor e; this quantity will increase as the velocity of the motor increases.

* So far as regards Dr. Moser's application of his theory to the carbon

Let e attain the value B F. Let us inquire what the actual current will be, and what the energy of it; also what the work done by the motor is.

First complete the construction as follows:—Through F draw F G H parallel to B C, and through G draw K G L parallel to A B. Then the actual electromotive force at work in the machine producing a current is $E-e$, which may be represented by any of the lines A F, K G, G H, or L C. Now the electric energy expended per second is EC; and since $C=\frac{E-e}{\Sigma R}$,

$$\frac{E(E-e)}{\Sigma R};$$

and the work absorbed by the motor, *measured electrically*, is

$$\frac{e(E-e)}{\Sigma R}.$$

ΣR being a constant, the values of the two may be written respectively

$$E(E-e)$$

and

$$e(E-e).$$

Now the area of the rectangle

$$AFHD=E(E-e),$$

and that of the rectangle

$$GLCH=e(E-e).$$

The ratio of these two areas on the diagram is the efficiency of a perfect motor, under the condition of a given constant electromotive force in the electric supply.

(2) So far we have assumed that the efficiency of a motor (working with a given constant external electromotive force) is to be measured electrically. But no motor actually converts into useful mechanical effect the whole of the electric energy which it absorbs, since part of the energy is wasted in friction and part in wasteful electromagnetic reactions between the stationary and moving parts of the motor. If, however, we

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photophone of Messrs Bell and Tainter, I entirely agree with him; my own experiments showing conclusively that the effects are due to heat only. But the best carbon cells are vastly inferior in their action to those of selenium.

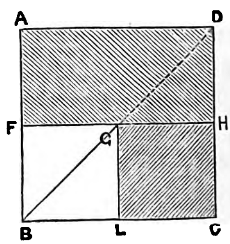
consider the motor to be a *perfect* engine (devoid of friction, not producing wasteful Foucault currents, running without sound, giving no sparks at the collecting-brushes, &c.), and capable of turning into mechanical effect 100 per cent. of the electric energy which it absorbs, then, and then only, may we take the electrical measure of the work of the motor as being a true measure of its performance. Such a "perfect" electric engine would, like the ideal "perfect" heat-engine of Carnot, be perfectly reversible. In Carnot's heat-engine it is supposed that the whole of the heat actually absorbed in the cycle of operations is converted into useful work; and in this case the efficiency is the ratio of the heat absorbed to the total heat expended. As is well known, this efficiency of the perfect heat-engine can be expressed as a function of two absolute temperatures, namely those respectively of the heater and of the refrigerator of the engine. Carnot's engine is also ideally reversible; that is to say, capable of reconverting mechanical work into heat.

The mathematical law of efficiency of a perfect electric engine illustrated in the above construction is an equally ideal case. And the efficiency can also be expressed, when the constants of the case are given, as a function of two electromotive forces. We shall return to this comparison a little later.

The Law of Maximum Rate of Working (Jacobi).

(3) Let us next consider the area G L C H of the diagram (fig. 2), which represents the work utilized in the motor. The value of this area will vary with the position of the point G, and will be a maximum when G is midway between B and D; for of all rectangles that can be inscribed in the triangle B C D, the square will have maximum area (fig. 2). But if G is midway between B and D, the rectangle G L C H will be exactly half the area of the rectangle A F H D; or, the useful work is equal to half the energy expended. When this is the case, the counter electromotive force reduces the current to half the strength it would have

Fig. 2.

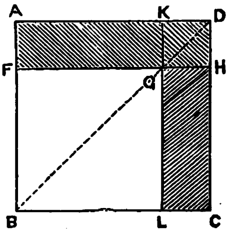


if the motor were at rest; which is Jacobi's law of the efficiency of a motor doing work at its greatest possible rate.

Law of Maximum Efficiency.

(4) Again, consider these two rectangles when the point G moves indefinitely near to D (fig. 3). We know from common geometry that the rectangle G L C H is equal to the rectangle A F G K. The area (square) K G H D, which is the excess of A F H D over A F G K, represents therefore the electric energy which is wasted in heating the resistances of the motor. That the efficiency should be a maximum the heat-waste must be a minimum. The ratio of the areas A F H D and G L C H, which represents the efficiency, can therefore only become equal to unity when the square K G H D becomes indefinitely small—that is, when the motor runs so fast that its counter electromotive force e differs from E by an indefinitely small quantity only.

Fig. 3.



Further, it is clear that if our diagram is to be drawn to represent any given efficiency (for example, an efficiency of 90 per cent.), then the point G must be taken so that area G L C H = $\frac{9}{10}$ area A F H D; or, G must be $\frac{9}{10}$ of the whole distance along from B towards D. This involves that e shall be equal to $\frac{9}{10}$ of E; which expresses geometrically the law of maximum efficiency.

It is strange that even in many of the accepted text-books this law is ignored or misunderstood. It is indeed frequent to find Jacobi's law of maximum rate of working stated as the law of efficiency. Yet as a mathematical expression the law has been known for many years. It is implicitly contained in more than one of the memoirs of Joule; it is implied also in more than one passage of the memoirs of Jacobi*; it exists

* Jacobi seems very clearly to have understood that his law was a law of maximum working, but not to have understood that it was not a law of true economical efficiency. In one passage (*Annales de Chimie et de Physique*, t. xxxiv. (1852) p. 480) he says:—" Le travail mécanique maximum, *ou plutôt l'effet économique*, n'est nullement compliqué avec ce que M. Müller appelle les circonstances spécifiques des moteurs électromag-

in the *Théorie Mécanique de la Chaleur* of Verdet*. Yet it remained a mere mathematical abstraction until its significance was pointed out three or four years ago by Siemens.

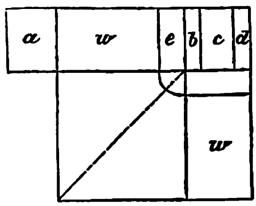
(5) Further, if the motor be not a "perfect" one, but one whose intrinsic efficiency, or *efficiency per se*, is known, the actual mechanical work performed by the motor can be represented on the diagram by simply retrenching from the rectangle G L C H the fraction of work lost in friction &c. Similarly, in the case where the electric energy expended has been generated in a dynamo-electric machine whose intrinsic efficiency is known, the total mechanical work expended can be represented by adding on to the area A F H D the proportion spent on useless friction &c. To make the diagram still more expressive, we may divide the area K G H D into slices proportional to the several resistances of the circuit; and the areas of these several slices will represent the heat wasted in the respective parts of the circuit. These points are exempli-

nétiques." Yet, though here there is apparently a confusion between the two very different laws, in a preceding part of the very same memoir Jacobi says (p. 466):—"En divisant la quantité de travail par la dépense (de zinc), on obtient une expression très-importante dans la mécanique industrielle: c'est l'effet économique, où ce que les Anglais appellent *duty*." Here, again, is a singular confusion. The definition is perfect; but "effet économique" is not the same thing as the maximum power. Jacobi's law is not a law of maximum efficiency, but a law of maximum power; and that is where the error creeps in. It is significant, in suggesting the cause of this remarkable conflict of ideas, that throughout this memoir Jacobi speaks of *work* as being the product of force and velocity, not of force and displacement. The same mistake—common enough amongst continental writers—is to be found in the accounts of Jacobi's law given in Verdet's *Théorie mécanique de la Chaleur*, in Müller's *Lehrbuch der Physik*, and even in Wiedemann's *Galvanismus*. Now the product of force and velocity is not work, but work divided by time—that is to say, rate-of-working, or "power." This may account for the widely-spread fallacy. Jacobi makes another curious slip in the memoir above alluded to (p. 463), by supposing that the strength of the current can only become =0 when the motor runs *at an infinite speed*. We all know now that the current will be reduced to zero when the counter electromotive force of the motor equals that of the external supply; and if this is finite, the velocity of the motor, if there is independent magnetism in its magnets, need also only be finite. This error—also to be found in Verdet—seems to have thrown the latter off the track of the true law of efficiency, and to have made him fall back on Jacobi's law.

* Verdet, *Œuvres*, t. ix. p. 174.

fied in fig. 4, which represents the transmission of power between two dynamos, each supposed to have an intrinsic efficiency of 80 per cent., each having 500 ohms resistance, working through a line of 1000 ohms resistance, the electromotive force of the machine used as generator being 2400 volts, and the counter electromotive force of the machine used as motor being 1600 volts.

Fig. 4.

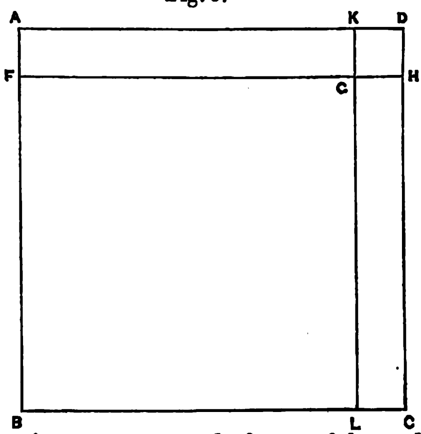


The entire upper area represents the total mechanical work expended. Call this 100, and it is expended as follows:—$a = 20$, lost by friction &c. in the generator; $b = 6\frac{2}{3}$, lost in heating generator; $c = 13\frac{1}{3}$, lost in heating line-wires; $d = 6\frac{2}{3}$, lost in heating motor; $e = 10\frac{2}{3}$, lost in friction in the motor; $w = 42\frac{2}{3}$ is the percentage realized as useful mechanical work.

(6) The advantage derived in the case of the electric transmission of power from the employment of very high electromotive forces in the two machines is also deducible from the diagram.

Let fig. 3, given above, be taken as representing the case where E is 100 volts and e 80 volts. Now suppose the resistances of the circuit to remain the same while E is increased to 200 volts and e to 180 volts. (This can be accomplished by increasing the speed of both machines to the requisite degrees.) $E - e$ is still 20 volts, and the current will be the same as before. Fig. 5 represents this state of things. The square K G H D which represents the heat-waste is the same size as befoer; but the energy spent is twice as great, and the useful work done is more than twice as great as previously. High elec-

Fig. 5.

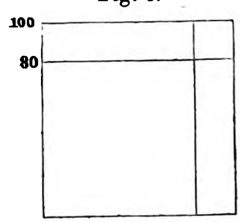


tromotive force therefore means not only a greater quantity of power transmitted, but a higher efficiency of transmission also. The efficiency of the system in the case of fig. 3 was 80 per cent.; in the case of fig. 5 it is 90 the dynamos used being supposed "perfect"); and whilst double energy is expended, the useful return has risen in the ratio of 9 to 4.

(7) So far it has been supposed that the resistance of the system is a constant quantity. But it is possible to construct diagrams in which changes of resistance are taken into account. All that is necessary is to vary the scale of the diagram, the linear unit of scale being chosen inversely proportional to the square root of the total resistance. This will make the areas of the diagrams inversely proportional to the resistances in the different cases, as required by the law that the energy of the current is proportional to $\frac{E^2}{R}$.

An example in which this rule is applied is the following. It can be shown that the power transmitted and efficiency of a transmitting system are increased by doubling the number of coils in the armatures of the machines. This is not at first sight self-evident; for though, *cæteris paribus*, this doubles the electromotive force of the machines, it also doubles their resistances. Let fig. 6 be the diagram for a transmitting system, where $e = \frac{4}{5}E$, and in which these values are both going to be doubled by doubling the number of armature-coils. There are two cases to consider:—(*a*) first, where the line-resistance is very small compared with that of the two machines; (*b*) second, where the line-resistance is very large compared with that of the two machines.

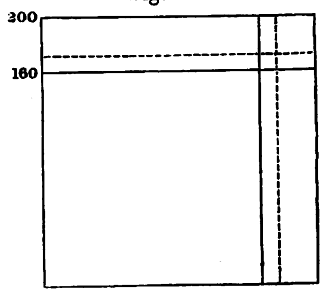
Fig. 6.



(*a*) In the former case, where we neglect the resistance of the line, we must draw a diagram diminishing the linear unit of scale to $\frac{1}{\sqrt{2}}$ of its value. But as on this scale we are going to represent doubled electromotive forces, the actual figure will have to be $\sqrt{2}$ times as large as fig. 6. Draw, then, the

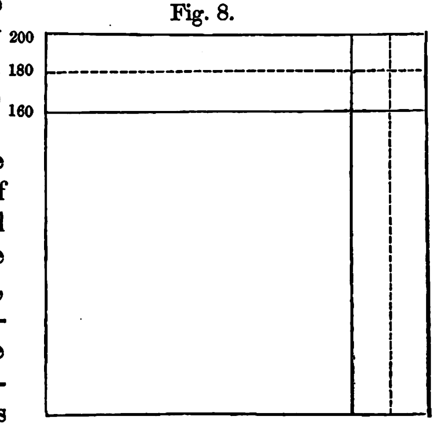
fig. 7, taking as side of the square a line equal to the diagonal of fig. 6, and we obtain a diagram in which, while the efficiency is the same as before, the actual quantity of work effected in unit time is doubled. For the areas representing respectively energy expended, work done, and heat-waste are in fig. 7 double of those in fig. 6. But no such case can occur in practice, as the line must have some resistance. Then doubling the number of coils of the machines will not cut down the scale so greatly as we have supposed; and the work transmitted will be more than doubled. Further, if the number of coils on the machine used as motor be a little more than doubled, a higher efficiency will be attained; since then the area of the square K G H D will be further diminished, while the scale on which the diagram is drawn will only be very slightly diminished. If diminished, as shown by the dotted lines, so that $e-E$ has the same value as before, the efficiency will be a little less than doubled, the power transmitted remaining as at first.

Fig. 7.



(*b*) If the case where the line-resistance is very great as compared with the resistances of the machines be taken, we find that doubling the number of the coils of the two machines will double their respective electromotive forces, without altering appreciably the total resistance or the scale of the diagram. To represent this change relatively to fig. 6, we must reconstruct that figure, doubling its linear dimensions each way, as in fig. 8. It is at once evident that the power transmitted is increased fourfold, while the efficiency remains the same. If we increase the

Fig. 8.



number of coils, as before, on the machine at the receiving end of the line so as to bring up the difference $E-e$ to the value it had in fig. 6, the scale of the diagram will still be unaltered; the power transmitted will be now only double instead of quadruple; but the efficiency will thereby be more than doubled, the heat-waste being the same, and the energy utilized more than twice as great. High electromotive force is therefore advantageous in both cases, especially in the case of a great resistance in the line.

(8) It only remains to point out a curious contrast that presents itself between the efficiency of a perfect heat-engine and that of a perfect electric engine. We saw (§ 2) that the one could be expressed as a function of two temperatures, whilst the other could be expressed as a function of two electromotive forces. But in the heat-engine the efficiency is the greatest when the difference between the two temperatures is a maximum; whilst in the electric engine the efficiency is the greatest when the difference between the two electromotive forces is a minimum. The two cases are contrasted in figs. 9 and 10, fig. 9 showing the efficiency of a heat-engine working between temperatures T and t (reckoned from absolute zero); whilst fig. 10 shows the efficiency of an electric engine receiving current at an electromotive force E, its counter electromotive force being e.

Fig. 9.

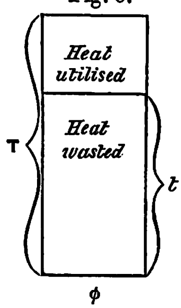
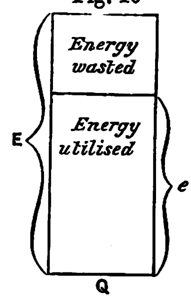


Fig. 10.



Joule's remark, here illustrated, that an electric engine may be readily made to be a far more efficient engine than any steam-engine, is amply justified by all experience. But in spite of this fact, electric engines are, as yet, dearer in practice than heat-engines, simply because energy in the form of electric currents supplied at a high potential is, as yet, much more costly to produce than energy in the form of heat supplied at a high temperature.

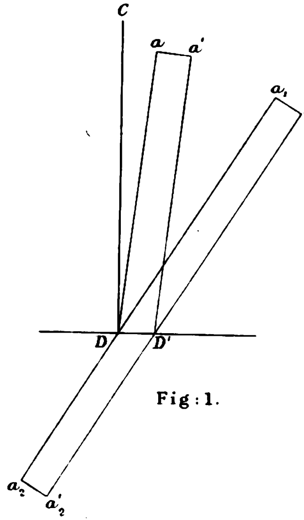


Fig: 1.

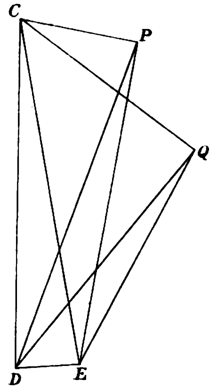


Fig. 2.

Fig. 3

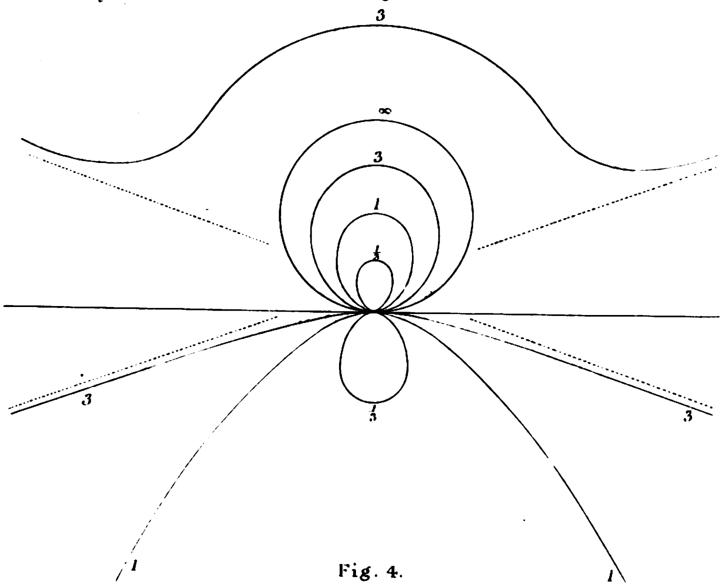
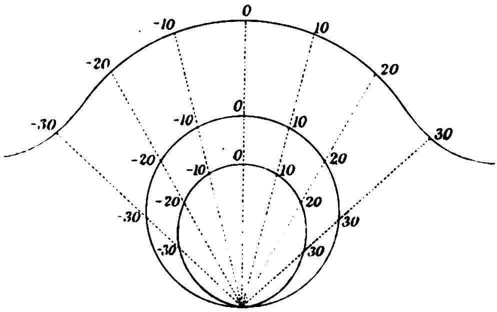


Fig. 4.



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XX. *On the Spectra formed by Curved Diffraction-gratings.* *By* WALTER BAILY*.

[Plate VIII.]

IN the curved diffraction-gratings invented by Professor Rowland, he has pointed out that if a source of light be placed at the centre of curvature, all the rays diffracted back from the grating will have their foci on the circle which lies in a plane perpendicular to the lines of the grating, and is described on the radius of curvature as diameter. In this paper I investigate the locus of these foci, and of those of rays transmitted through a transparent grating for any position of the source of light in the same plane.

Let a plane grating be placed at D (fig. 1) with its lines perpendicular to the paper and one of them passing through D, and its plane perpendicular to C D, and let a D be an incident ray, of which a portion with wave-length λ_1 is diffracted along $\mathrm{D}a_1$, and a portion with wave-length λ_2 is diffracted along $\mathrm{D}a_2$, $a_1\mathrm{D}a_2$ being a straight line. Let $\mathrm{CD}a=\theta'$, and $\mathrm{CD}a_1=\theta$; and let σ be the distance between the lines of the grating. Let D′ be the next line of the grating to D; and draw $\mathrm{D}'a'$ and $a'_1\mathrm{D}'a'_2$ parallel to $\mathrm{D}a$ and $a_1\mathrm{D}a_2$ respectively; and draw aa', $a_1a'_1$, $a_2a'_2$ perpendicular to $\mathrm{D}a$ and $a_1\mathrm{D}a_2$. Then we must have

$$a\mathrm{D}+\mathrm{D}a_1=a'\mathrm{D}'+\mathrm{D}'a_1'+n_1\lambda_1$$

and

$$a\mathrm{D}+\mathrm{D}a_2=a'\mathrm{D}'+\mathrm{D}'a'_2+n_2\lambda_2,$$

where n_1 and n_2 are integers.

These equations give us

$$\sigma(\sin\theta'+\sin\theta)=n_1\lambda_1,$$
$$\sigma(\sin\theta'-\sin\theta)=n_2\lambda_2.$$

Now (fig. 2) let D be the centre of a cylindrical grating whose lines are perpendicular to the plane of the paper, C the centre of curvature, and CD$=c$. Let P be the source of light, Q the focus of a diffracted ray, E a point on the grating near to D. Join PD, PE, QD, QE, CE. Let CP$=a$, CQ$=b$, $\angle$PCD$=\alpha$, QCD$=\beta$; DP$=r$, DQ$=r'$, $\angle$CDP$=\theta$,

* Read January 27, 1883.

$\angle CDQ=\theta'$, $\angle DCE=\gamma$. Then we have for light diffracted back from the grating, which we may call "reflected light,"

$$\sin CEP+\sin CEQ=n\frac{\lambda}{\sigma},$$

$$\frac{a\sin(\alpha-\gamma)}{\{a^2+c^2-2ac\cos(\alpha-\gamma)\}^{\frac{1}{2}}}+\frac{b\sin(\beta-\gamma)}{\{b^2+c^2-2bc\cos(\beta-\gamma)\}^{\frac{1}{2}}}=\frac{n\lambda}{\sigma}.$$

Expanding in terms of γ, we get

$$\frac{a\sin\alpha}{\{a^2+c^2-2ac\cos\alpha\}^{\frac{1}{2}}}+\frac{b\sin\beta}{\{b^2+c^2-2bc\cos\beta\}^{\frac{1}{2}}}-\frac{n\lambda}{\sigma}$$

$$+\left[\frac{(a^2+c^2-2ac\cos\alpha)a\cos\alpha-a^2\sin^2\alpha}{(a^2+c^2-2ac\cos\alpha)^{\frac{3}{2}}}\right.$$

$$\left.+\frac{(b^2+c^2-2bc\cos\beta)\cos\beta-b^2\sin^2\beta}{(b^2+c^2-2bc\cos\beta)^{3}}\right]\gamma$$

$$+\text{ terms involving higher powers of }\gamma=0.$$

Putting

$$a\cos\alpha=c-r\cos\theta,\qquad b\cos\beta=c-r'\cos\theta',$$

$$a\sin\alpha=c\sin\theta,\qquad b\sin\beta=c\sin\theta',$$

we get

$$\sin\theta+\sin\theta'-n\frac{\lambda}{\sigma}+\left[\frac{\cos\theta}{c}-\frac{\cos^2\theta}{r}+\frac{\cos\theta'}{c}-\frac{\cos^2\theta'}{r'}\right]c\gamma+\&c.=0.$$

This equation must be satisfied for all very small values of γ. Hence

$$\sin\theta+\sin\theta'=\frac{n\lambda}{\sigma};$$

and

$$\frac{\cos^2\theta}{r}=\frac{\cos\theta}{c}+\frac{1}{d},$$

$$\frac{\cos^2\theta'}{r'}=\frac{\cos\theta'}{c}-\frac{1}{d},$$

where d is any quantity.

In the last equation put $180+\theta'$ for θ', and $-r'$ for r'. The equation then becomes identical with the corresponding equation. Hence curves whose equation is

$$\frac{\cos^2\theta}{r}=\frac{\cos\theta}{c}+\frac{1}{d}$$

have the property that, if the source of light is at any point on one of these curves, the whole of the reflected spectra produced by the grating lie on the same curve.

If we start with the equation for transmitted light,

$$\sin\theta' - \sin\theta = n\frac{\lambda}{\sigma},$$

we shall arrive at the same result. Hence we see that each of the curves whose equation has just been found is the locus of the foci of all the diffracted rays, when the source of light is at any point on the curve.

These curves I will call "diffraction-curves." It is obvious from the equation that they are independent of the distance between the lines of the grating.

A table is given at the end of the paper showing the values of r for every five degrees in the value of θ, d having the values $\frac{c}{3}$, c, $3c$, c being taken as 1000; and the forms of the curves are shown in fig. 3.

When d is infinite, the diffraction-curve is a circle having the radius of curvature of the grating as diameter, and a straight line through D tangential to the grating. In every other case the curve is formed of two loops, one lying inside the circle and the other outside, touching one another at D. The inner loop is always an oval, which is infinitely small when d is zero, and increases as d increases, until d becomes infinite, when the inner loop coincides with the diffraction-circle. The outer loop is finite when d is less than c; and increases as d increases, until d equals c, when the outer loop becomes infinite, and resembles a parabola. When d is greater than c the outer loop takes somewhat the form of an hyperbola, with the asymptotes inclined to the axis at an angle whose cosine is $\frac{c}{d}$, and intersecting one another at a distance from the grating $=\frac{c^3}{d^2-c^2}$. One of the two branches into which the outer loop is now divided passes through D, always retaining the resemblance to a branch of an hyperbola, and ultimately, when d is infinite, becomes a straight line tangential to the grating. The other branch has points of inflection, if d is greater than $\frac{3}{2}c$, in the positions for which $3c\cos\theta = d - \sqrt{18c^2+d^2}$; and when d is greater than $2c$, this branch has points which are at a minimum distance from D. At these points the dis-

tance from D is $\frac{4c^2}{d}$, and $\cos\theta = -\frac{2c}{d}$. Consequently the locus of these points is the circle of curvature of the grating. When d becomes infinite, this branch coincides with the tangent to the grating at D, and with the diffraction-circle.

The diffraction-curve has been shown to consist of two loops, one of which passes through the source of light. This loop is the locus of the spectra of transmitted light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma(\sin\theta' - \sin\theta).$$

The other loop is the locus of the spectra of reflected light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma(\sin\theta' + \sin\theta).$$

As both loops coincide in the diffraction-circle, this circle is the locus both of the spectra of transmitted and of reflected light when the source of light is on the circle.

As an example of the determination of the wave-length, suppose the grating to have 25,000 lines to the inch; then each division of the grating is 40 millionths of an inch. Divide the diameter of the diffraction-circle into 40 parts, and with the centre of curvature as centre describe circles through these divisions, and number the points in which they cut the diffraction-circle, beginning with the centre of curvature as zero, and counting the readings as positive on one side of the zero and negative on the other (see fig. 4, in which only every tenth reading is given). If the source of light be at the centre of curvature, the readings of the diffraction-circle will give the wave-lengths, or multiples of them, in millionths of an inch. Now with the centre of the grating as centre of projection, project the readings of the diffraction-circle on both branches of any diffraction-curve, and place the source of light at the point in which one of the loops cuts the perpendicular from the centre of the grating. The readings give the wave-lengths, or multiples of them, as before.

Let the source of light be now placed at any point of a graduated diffraction-curve. Take the reading of the point, and *subtract* it from the readings of all other points on the *same* loop; the new readings will give the wave-lengths, or mul-

tiples of them, for transmitted light. *Add* the reading of the position of the source of light to the readings of all points on the *other* loop, and the new readings will give the wave-lengths, or multiples of them, for reflected light. The diffraction-circle must be treated as two distinct loops, and the reading of the source be subtracted from the readings on the circle for transmitted, and added for reflected light. One of the zero-readings occurs at the source of light, and the other at the focus of ordinary reflected light.

In the case of a plane grating, since c is infinite, the equation to the diffraction-curve becomes

$$r = d \cos^2 \theta.$$

TABLE showing the values of r for every five degrees of θ in the equation

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d}.$$

$\pm\theta$.	$d=\frac{c}{3}$.	$d=c$.	$d=3c$.	$d=\frac{c}{3}$.	$d=c$.	$d=3c$.	$\pm\theta$.
90	0	0	0	0	0	0	90
85	2	7	18	3	8	31	95
80	9	20	77	11	37	190	100
75	21	53	113	25	90	899	105
...	...	...	...	...	...	13691	109
70	35	87	173	44	178	−13451	110
65	52	126	231	73	309	− 2000	115
60	71	167	300	100	500	− 1500	120
55	92	209	363	140	772	− 1372	125
50	113	252	423	175	1156	− 1335	130
45	135	293	481	224	1707	− 1338	135
40	156	332	535	263	2508	− 1356	140
35	176	369	582	314	3710	− 1382	145
30	194	402	625	351	5603	− 1408	150
25	210	431	663	392	8768	− 1434	155
20	224	455	694	429	14640	− 1456	160
15	235	475	718	459	27394	− 1475	165
10	243	489	736	481	63890	− 1489	170
5	248	497	746	495	261165	− 1497	175
0	250	500	750	500	∞	− 1500	180

$c = 1000.$

XXI. *Optical Combinations of Crystalline Films.* *By* Lewis Wright.

[Plate IX.]

The object of the following experiments is to illustrate the facility with which simple combinations of mica-films, such as can be readily put together by any one with the aid of Canada balsam dissolved in benzol, may be made to demonstrate not only the simpler chromatic phenomena of polarized light, but also the more beautiful and complicated appearances encountered at a more advanced stage of study. The colours obtainable from such mica-films are more delicate and intense than the usual selenite preparations, because while in selenite those films which produce the lower and more intense orders of Newton's colours are so thin as to be split with difficulty, in mica they can be obtained with the greatest facility. Some of the preparations are also, as demonstrations, superior in themselves.

Let us take first the simplest case, of different retardations produced by different thicknesses of crystal, counteracted or not by opposite retardations caused by another crossed film. It has been usual to demonstrate these by two selenite wedges, rotating one over the other. A simpler and more effective demonstration is given by two wedges, each built up of similar mica-films superposed, and cemented together, like those on the screen (Pl. IX. fig. 1). The series of flat steps or tints are both more conspicuous and more readily understood; and if the two wedges are properly matched, when crossed the diagonal row of squares will be black when the Nicols are crossed. When the thick edge of one is superposed over the thin edge of the other, with the mica axes parallel, we have an even tint; and when the thick edge is superposed over the other thick edge, with the mica axes crossed, the retardations or colours produced by the first wedge are all destroyed by the second, and the field is all black. I wish to remark here, as I have done elsewhere, that the idea of wedges and other designs, built up in this way of thin flat films, is not due to me, but to my friend Mr. Fox, F.R.M.S., from whom both this pair of wedges and the next one are simply copied.

Here is another preparation of the same character, built up

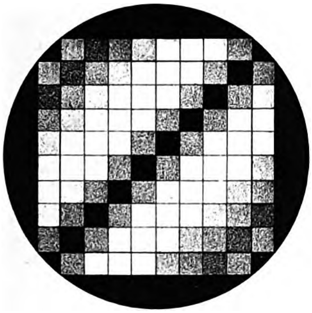


Fig. 1.

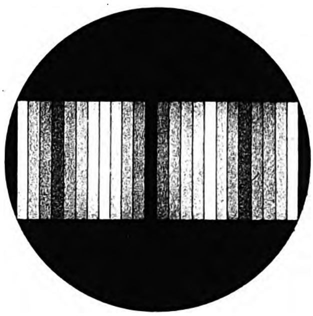


Fig. 2.

Fig. 3.

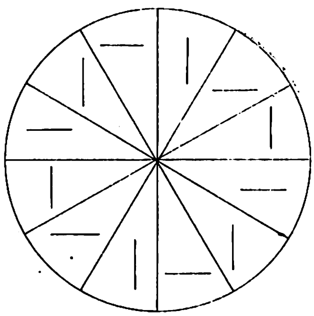


Fig. 4.

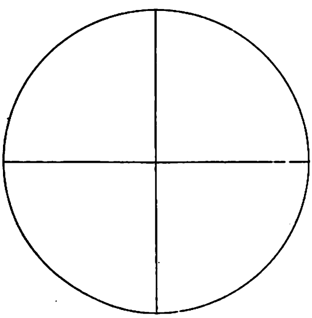


Fig. 5.

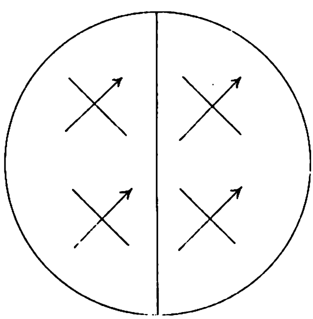


Fig. 6.

Mintern Bros lith.

of twenty-four films, each of a thickness causing exactly $\frac{1}{8}\lambda$ of retardation for yellow light. Of course the thickness must be very exact to bear multiplication twenty-four times without sensible error. Now if we superpose on this wedge a flat plate of mica with its axis* crossing that of the wedge, and of a thickness equal to the middle stripe, that central stripe must appear black when the Nicols are crossed (fig. 2), while Newton's first order of colours and half the second order appear symmetrically on both sides of the black stripe. On one side of the stripe the wedge itself gives greater successive thicknesses; while on the other side the plate of mica does the same.

Such a wedge has further and real optical uses. It shows at a glance the precise composition of every successively increased $\frac{1}{8}\lambda$ of retardation for the first three orders of Newton's colours. When extinction is complete for yellow light, we know that a little of both red and blue must be unextinguished, the two giving us at the end of the first order the opaque plum-colour known as the "tint of passage." As the red at a given distance from the end of the spectrum is visually more conspicuous than the blue, at the end of the second and third orders this "tint of passage" must become more and more red, as we see on the screen is the case. The precise composition of the light destroyed and that remaining, we may demonstrate by placing a slit across the wedge and throwing its spectrum on the screen (fig. 3), when we see the shifting of the bands in steps for each $\frac{1}{8}\lambda$ of retardation. The wedge alone shows only the first three orders; but it is obvious that by superposing a plate of mica 1λ in thickness, the spectrum would give us from the second to the fourth orders, and so on. I have not here the plates to show this in detail; but I have brought a thick plate of selenite, not measured, but hurriedly mounted for this afternoon. We throw its spectrum on the screen first: from the seven or eight dark bands it appears to be from eight to ten waves in thickness; it is at all events so thick as to show no colour. But now superposing the wedge, the shifting of the bands shows the precise composition for every successive $\frac{1}{8}\lambda$ of retardation even in this high

* Throughout this paper the "axis" of the mica is supposed to be that one of its two polarizing planes which passes through the two optic axes.

order of interferences. It is all rendered by spectrum analysis. Another great use of such a wedge is for gauging the thickness of films in making other preparations, for which I use it constantly: we only have to superpose the film to be gauged with its axis crossing that of the wedge, and the stripe that is nearest extinction when the Nicol is crossed gives the thickness.

The *rotatory* colours of films are also beautifully shown by mica preparations. We all know that if a film $\frac{1}{4}\lambda$ thick (the terms "thick" or "thickness" of course mean in retardation of the slowest ray, throughout this paper) is adjusted with its polarizing planes at 45° with the plane of polarization, we obtain a single circular vibration. But if we adjust in this position a film giving colour next the polarizer, and introduce after that the $\frac{1}{4}\lambda$ plate, with its planes at an angle of 45° with those of the colour-film, both the two rays which emerge from the first film are converted into rays circularly polarized, but in opposite directions; and hence we get approximately the rotatory colours of quartz as the analyzer is rotated. The geometrical figure I now insert is thus circularly polarized, and will illustrate not only the beautiful rotational phenomena of the colours, but also that superior delicacy and intensity of these lower-order colours which has been alluded to: it would be exceedingly difficult to get colours like these in selenite. Again, we take the 24-section wedge used just now, and superpose upon it a $\frac{1}{4}\lambda$ plate made in two halves, one of which has its planes reversed as compared with the other; on rotating the analyzer the colours appear to pass along the wedge in opposite directions, as if it were made in two halves of right- and left-handed quartz.

My friend the Rev. P. R. Sleeman lately suggested to me another preparation, which was in turn suggested to him by a beautiful one in quartz belonging to the President of the Royal Society. This is a quarter-wave plate divided into twelve sectors. In the position now on the screen the polarizing planes are all perpendicular and horizontal; but the principal plane or "axis" is *reversed* (as in fig. 4) in every alternate sector. If we superpose this upon a mica-film giving uniform colour, on rotating the analyzer we get, as you see, the contrary quartz rotations. But it lately occurred to me that a still more beautiful demonstration of these rotational

colours would be obtained by another combination, which deserves perhaps to be called an "optical chromatrope." We place first in the stage next the polarizer a large even-tint film in a rotating frame; next to that a concave selenite plate showing Newton's rings; next to that again our quarter-wave plate in sectors. As we rotate the analyzer, one set of alternate sectors of the rings approach the centre, while the intermediate sectors recede from it; and if we now at the same time rotate the even-tint plate, we simultaneously vary the *colour* phenomena in an exquisitely beautiful manner.

A $\frac{1}{4}\lambda$ plate divided into four sectors or quadrants, with their planes alternately reversed in the same way (fig. 5), enables us to demonstrate the nature of the curious modifications of the rings and brushes in a plate of crystal when circularly-polarized convergent light is employed. Here, for example, are the rings and cross of calcite: interposing a $\frac{1}{4}\lambda$ plate, the black cross disappears into a grey nebulous one, and on opposite sides of each arm the quadrants of rings appear dislocated, the dark rings of one quadrant opposing the light rings of its neighbours. Interposing another $\frac{1}{4}\lambda$ plate on the other side, on rotating the analyzer one opposite pair of quadrants contracts while the intermediate ones expand, so that in two complementary positions we have unbroken circles. The same phenomena precisely are exhibited by this disk of chilled glass in parallel light, the gradually decreasing elasticity of the glass as we recede from the centre having the same effect as the increasing convergence of the rays has in the calcite. Now it is pretty easy to explain this phenomenon to a student by such a diagram as this (fig. 6) representing our crystal or glass with the Nicols crossed. The circularly-polarized ray we know is, on entering the glass, decomposed into its two plane-polarized components, of which one (let us suppose that denoted by the arrow-heads) is retarded a quarter of a wave. But the calcite or glass, beside this, itself also retards either the radial or the tangential vibration more than the other component—in calcite the radial. Taking, then, any originally-circular ring caused by the calcite retardations alone, we see that in two opposite quadrants the $\frac{1}{4}\lambda$ plate retards the radial vibrations a further quarter-wave, while in the alternate quadrants it accelerates them a quarter-wave. The result must

obviously be a half-wave dislocation. As I have just observed, such a diagram sufficiently explains it all; but it seems to me better actually to represent it optically, by introducing the composite quarter-wave plate, with its planes at 45° with the plane of polarization, before a film ground concave to show Newton's rings. Here we have, in an analogous way, in opposite quadrants retarded one of the component vibrations a quarter-wave before entering the selenite, while in the alternate quadrants we retard the other component; and we get similar dislocations. Again, letting the concave selenite come first, and superposing a $\frac{1}{4}\lambda$ plate cut in quadrants with their planes alternately horizontal and vertical, we now have the contracting and expanding quadrants, with the perfect circles in two positions, as in the calcite. We may make the demonstration complete by reversing the process, and superposing our last composite $\frac{1}{4}\lambda$ plate on the disk of chilled glass*. We now are applying in each quadrant all the retardations equally to either the tangential or radial vibrations; and hence the rings remain perfectly concentric, while they expand or contract as the analyzer is rotated: there is no dislocation at all. Finally, either the quadrant or 12-sector $\frac{1}{4}\lambda$ plate superposed on this *square* of chilled glass gives us a very beautiful demonstration that the dislocation of the crystal rings is entirely due to the $\frac{1}{4}\lambda$ plate retarding one component ray in the crystal on one side of the plane of polarization or that at right angles to it, and accelerating the same component on the other side of those planes. Here we have the square perpendicularly adjusted, with the composite plate superposed. When the analyzer is rotated, the reversal of the sectors on the lines of the black cross keeps the figure symmetrical, as in the last experiment. But you observe that the *diagonals* of the square are covered, each by a single plate or sector; and a mere glance at the screen makes it obvious that if, *in this position*, these diagonals were covered, as the black cross now is, by the junction-line between two contrary sectors, they would be dislocated, the colours on one

* In private experiment we can of course do this with a plate of calcite; but in a projecting instrument it is rather difficult to ensure the precise axial coincidence of all the arrangements with the axis of the convergent light, without which the experiment fails.

side of the line approaching the centre, and those on the other receding when we rotate the analyzer. But we will now bring these diagonals of the square into the planes of polarizer and analyzer crossed, and superpose the sectors again upon the glass, junction-lines now covering the diagonals. You observe that the state of things is exactly reversed; and the contrary sectors now do keep the figure symmetrical on each side of the diagonals, while, on the other hand, the single $\frac{1}{4}\lambda$ plates which now cover the bisecting diameters of the square preserve the symmetry there also. It is not necessary to add details of explanation which will be familiar to all.

Allow me next to illustrate the beautiful phenomena of crossed films of mica in highly convergent light, such as will take in biaxial angles of, say, 50°. Our starting-point will be Norremberg's beautiful discovery, worked out entirely from theory, that by crossing films of biaxial mica of gradually increasing number and proportionately diminished thickness, there was a gradual approach to the rings and cross of a uniaxial crystal. He found three wave-lengths of retardation the best approximate unit. Here is a single plate of mica—the ordinary biaxial lemniscates; and here are two such plates crossed at right angles—the ordinary figure of a "crossed" crystal, in which we get the black cross. With four plates crossed we get the first approach towards rings, each of the "eyes" being now bisected by a straight fringe placed as a tangent to the figure. Norremberg's next preparation was eight films crossed; but I add one of six, which gives a single perfect though nearly square ring, while eight films give two rings. Twelve give three rings and signs of a fourth; while twenty-four, as you see, are absolutely undistinguishable from a calcite. The whole series will be thus:—

$$\frac{1}{3\lambda},\ \frac{2}{3\lambda},\ \frac{4}{\frac{3}{4}\lambda},\ \frac{6}{\frac{1}{2}\lambda},\ \frac{8}{\frac{3}{8}\lambda},\ \frac{12}{\frac{1}{4}\lambda},\ \frac{24}{\frac{1}{8}\lambda}.$$

Now there is no necessity for an *exact* total thickness of three wave-lengths in constructing this series; but an approximation to it is necessary, to preserve the gradation of the phenomena and the gradual passage to the uniaxial figure. So far Norremberg ascertained; but he does not seem to have carried his experiments with mica any further. Let us now do so.

The eight films gave us two rings, the outer one squarish in figure. But if we combine eight very *thin* films (say $\frac{1}{8}\lambda$ thick, as in this preparation), you observe that we get perfectly circular rings at once; and in fact even four very thin films will give them; and twelve thin films give us quite fine circles. Now, on the other hand, let us employ four and eight *thick* films—in this case over 1λ thick (we thus more than double Norremberg's thicknesses); and observe that the rings now have altogether disappeared, and the curved fringes are all turned the reverse way, their convex sides to the centre. The same thing is still more evident in this splendid figure, produced by twelve crossed films $\frac{3}{4}\lambda$ in thickness. We see easily enough that it must be so, if we follow in our minds the decompositions and recompositions of the vibrations in traversing the successive films; but it is very interesting to notice how, with the same number of micas crossed in exactly the same way, but of different thicknesses, the phenomena appear actually reversed in character. Having seen this, we abandon simply crossed films, and the following will be composed of films superposed at angles of both 90° and 45°. Here another cause of variety comes into play, since all films whose thickness contains an odd $\frac{1}{4}\lambda$, when superposed at 45° will circularly polarize the light. Moreover we also know that if two such films are superposed at an angle of 45°, the effect is to rotate the plane of polarization itself (as shown for instance by the rotation of a calcite cross) 45° from the original plane. Hence the variety and scope for combination here are endless, the phenomena always being beautiful; but I must only show you a very few of such preparations. The first four are all composed of films $\frac{1}{2}\lambda$ in thickness, and each contains the same number of twelve films, and the lines show the successive positions of the mica "axis." In the first they are

| ⟍ | — ⟍ — | ⟍ | — ⟍ —

Thus all the diagonal axes lie the same way. Now this second preparation has the very same individual films differently placed, thus:—

| ⟍ — | ⟍ — | ⟋ — | ⟋ —

You see the total difference in effect produced by the difference in crossing.

The next one is thus arranged:—

| | — | | — | = | ≡

This is an interesting combination, because the wave-decompositions indicate that the light should be nearly extinguished when the Nicols are crossed, not only in the original black cross, but also along the diagonals between. You see that it is so; but this result is still more completely brought out by the next preparation,

| — | — | — — | — | — |

where we get a nearly perfectly black square crossed by nearly black diagonals as well as by the black cross. The next set of five are all built of films one wave in thickness, as follows:—

No. 1.	8 micas	| — | × | — |	
2.	10 ,,	+ + × + +	
3.	12 ,,	+ + × × + +	
4.	8 ,,	+ × + ×	
5.	8 ,,	| ╱ — ╲ | ╱ — ╲	(*i. e.* successively rotated 45°).

This last figure is interesting, because we can see that the result must be some polygonal or roughly circular central figure with some sort of a cross, surrounded by eight detached figures or eyes. I am sorry I cannot work this out mathematically; but with whole-wave and somewhat greater retardations it is pretty easy to trace it in one's own mind. It is so, as you perceive; and you also see that the preparation and figure can be rotated without very sensible change, which also follows from theory, and is a somewhat remarkable result, after what we saw at the commencement, with such thick films. And now, to show the effect of thickness, here is a precisely similar preparation of eight films superposed at a successively rotated angle of 45°, but built up of $\frac{3}{4}\lambda$ films. Circular polarization here comes into play; and the effect is totally different in every way. The last of these crossed micas is built up of twelve $\frac{3}{4}\lambda$ films, thus—

× + + + + ×

You see the total difference in figure from any thing before, and the scope for endless variety, which I must not further pursue.

Still more beautiful, but perhaps less interesting, are the

combinations of mica- and selenite-films discovered experimentally by Norremberg. As he observes, if we call the three axes of elasticity in any crystal x, y, z, then selenite-films contain x and z, while mica on the other hand contains y and z; and it is easy to see that if preparations are built up of both elements, very fine coloured fringes must result, differing very greatly in character according to whether the x of the selenite is parallel to the z of the mica or crosses it. As far as I remember, however, Norremberg and Reusch seem to have said that the characters of the fringes defy all prediction. This is perhaps hardly true, even apart from mathematical analysis, which I am unable to give, and which the mere beauty of these combinations is scarcely worth. For it is easy to perceive that if a single selenite be placed between two thick micas, we must have very nearly the usual biaxial figure, with some little modification in the eyes or rings, but chiefly distinguished from the simple mica by rich colour. That is so here. But if we alternate several parallel selenites between parallel micas of less thickness, so as to give the selenite functions of elasticity more comparative influence, then it is evident that the modified lemniscate curves, or what is still traceable of them, must be either brought nearer together or more widely separated, and that we shall thus obtain curved fringes having their approximate origin in the original optic axes of the mica, but reversed in character according as the x of the selenite crosses or is parallel to the mica z. This is the simplest analysis I can give, and it follows on consideration from Norremberg's data. Here are two such preparations, in each of which four selenites are alternated between five $\frac{1}{2}\lambda$ micas. In the first the modified lemniscates are wider apart than in the mica, but the resulting fringes originate approximately in the mica axes. In the second the selenites are at right angles with their former position, and the fringes still centre in the axes, but the curves are reversed; and the resulting "palm-tree" pattern is perhaps one of the most beautiful, both in colour and figure, which it is possible to behold.

The few other preparations here are built up of either four or six ternary elements constructed on Norremberg's system, each consisting of two parallel micas, with a selenite between

them either crossed or parallel. In the first you readily recognize the "palm-tree" character of the last figure, "crossed." It is needless to describe the others; for here, too, variety is boundless; but I purposely reserve for the last, two combinations composed of exactly the same arrangements of both mica and selenite, and all the micas the same ($\frac{1}{2}\lambda$) thickness, but the selenite films in one slightly thicker than in the other. The difference in effect is purposely not so great as to prevent your recognizing the same general figure in both, but is still conspicuous and interesting. Let me, in conclusion, hope that the beauty of these preparations constructed after Norremberg's method, and the facility with which they can be prepared, may make them better known.

[At the conclusion of the paper Mr. Wright described and exhibited an adaptation to the microscope by Messrs. Swift and Son, by the aid of which all the preparations and crystals requiring highly convergent light could be shown on the stage of any microscope constructed with a draw-tube.]

XXII. *On a Method of Measuring Electrical Resistances with a Constant Current.* *By* SHELFORD BIDWELL, *M.A., LL.B.**

IT sometimes happens that the resistance of a body appears to depend upon the strength of the current which traverses it. Thus the resistance of the carbon filament of an incandescent lamp may be several ohms lower when tested with a strong current than it is with a weak one. In this case there is little doubt that the difference is due only indirectly to the current itself, and is in fact caused by the heat which the stronger current develops, and which, even when the circuit is closed only for a moment, may produce considerable effect upon the conductivity of the filament. Again Prof. Adams, at an early stage of his well-known experiments with selenium, found that, on increasing the strength of the current through the selenium, there was a diminution in its resistance †. The same is the case with the mixtures of sulphur and carbon which I described in a previous

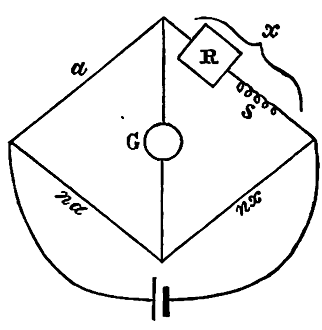
* Read March 10, 1883.

† Phil. Trans. vol. clxvii. pp. 319, 342.

communication *, and to a very much greater degree with loose contacts of carbon or metal, such as are used in the microphone. For example, a carbon pencil being arranged so as to rest at right angles upon another with a pressure of ·05 grm., the resistance at the point of contact was found to be 11·02 ohms with a current of ·1 ampère, and 68 ohms with ·001 ampère; and when cylinders of bismuth were substituted for the carbon, the resistances with the same currents as before were 5 ohms and 182 ohms respectively.

Without assuming that the resistances in these and similar cases are altogether true resistances, it is nevertheless sometimes convenient to treat them as such; and for purposes of comparison it is clearly necessary that currents of known or constant strength should be used in their measurement. When the Wheatstone's bridge is employed in the usual manner, the current passing through the unknown resistance will, of course, vary with the magnitude of this resistance, being smaller when it is high than when it is low; but by a very simple modification of the common arrangement, which I have used extensively during the last year, it is easy to ensure having currents of uniform strength throughout a series of measurements.

In the figure, x, nx, a, na are the four arms of a Wheatstone's bridge, S is the unknown resistance, and R is a box of



resistance coils which is inserted in the same arm. If E denote the electromotive force of the battery, B its internal resistance, and C the current which passes through the arm

* Proc. Phys. Soc. vol. v. p. 90.

containing S, then, when there is a balance,

$$C=\frac{n}{n+1}\times\frac{E}{B+\frac{n(a+x)^2}{a+x+n(a+x)}}$$
$$=\frac{nE}{(n+1)B+n(a+x)}.$$

From this expression we can find what value x must have in order that the current through the unknown resistance may be of any definite strength. Having determined this value, we insert resistance equal to n times its amount in the arm nx, and adjust the resistance in the box R until a balance is obtained. We then know that the resistance of R + S is equal to x; that the resistance required to be measured, S, is equal to that of the arm x less the resistance employed in R; and that a current of the desired strength, C, is passing through it. A second unknown resistance may now be substituted and measured as before, simply by altering the resistance of R, with the certainty that when there is a balance the current is of the same strength as in the former case. The resistance nx remains unchanged throughout. It is of course necessary so to choose the values of a, n, and E that x may be greater than the resistance to be measured; and it is generally desirable that the resistance of the whole bridge should be made as high as conveniently possible.

The great advantage of this method over others that suggest themselves lies in the fact that, since it is never necessary to close the circuit for more than a moment, the electromotive force and resistance of the battery remain sensibly constant during a long course of experiments.

XXIII. *The Resistance of the Electric Arc.*

By Professors W. E. Ayrton, *F.R.S.*, *and* John Perry, *M.E.**

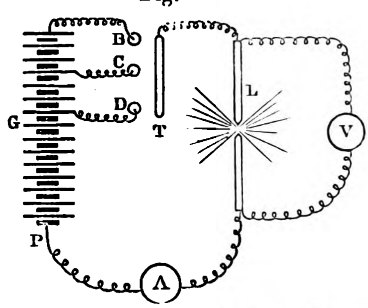
One of the results of the elaborate set of experiments on the Electric Light conducted in 1878 by the late Mr. Schwendler was the conclusion to which he came, that the supposition that the resistance of an arc of constant length

* Read December 9, 1882.

was inversely proportional to the current which passed through it was highly probable. His experiments, however, were not sufficient to absolutely determine this point; and it has therefore appeared to us important to obtain further information on the subject, which, with the assistance of the students working in our laboratory, we have from time to time done with the following results.

1. The method employed by us in the first instance was as follows:—A number of Grove cells, G, were arranged in series (fig. 1); and one pole P was connected through an ammeter A with one carbon of the electric light L, the other carbon of which was attached to a mercury trough T, which, by means of a metallic bridge-piece, could be connected with any one of the mercury-cups B, C, D, each of which was permanently electrically connected with the terminal of a different number of cells. The two carbons were also connected with the terminals of a voltmeter V, by means of which the difference of potentials between the carbons at any moment could be determined. The experiment was made thus:—The bridge-piece was put into D, and the carbons by means of a rack adjustment separated until a good steady light was obtained, when readings of the ammeter and voltmeter were taken. A second bridge-piece was now put into C and that in D quickly withdrawn, the effect being to suddenly increase the number of cells in circuit producing the light, increasing therefore the current without interrupting it, and without changing the distance between the carbons, as the lamp had no automatic adjusting arrangement. Readings of A and V were then quickly taken and the operation reversed—that is, the bridge-piece put into D and that in C quickly withdrawn, which had the effect of again reducing the current; and if the change back again were effected not long after the first, the carbons were not sufficiently burnt away by the stronger current to make the light go out when the

Fig. 1.



current was reduced, so that a third set of readings of A and V could be taken. In this way, for the same distance between the carbons two readings of the lower current and its corresponding carbon difference of potentials, and one intermediate reading of the higher current with its carbon difference of potentials, were obtained. The whole experiment was now repeated with the cells P C and P B instead of P D and P C. The following is a sample of the results obtained from a number of tests with 30, 40 and 50 Grove cells :—

Number of cells.	Current, in ampères.	Difference of potentials between carbon, in volts.	Work in foot-pounds per second in arc.
30	6·52	30·4	146·2
40	10·16	30·4	227·8
50	11·92	30·4	267·2

The last current is therefore nearly double the first, but the difference of potentials between the carbons is not materially altered by the increase of the current and the light.

Subsequently a large number of experiments were made using a Brush dynamo in place of the Grove cells, and increasing and diminishing the current by suddenly increasing and diminishing the resistance in circuit without stopping the current.

In the earlier experiments for each current its value was read on the ammeter as well as the difference of potentials between the carbons on the voltmeter; but since even with a very dead-beat ammeter some little time must elapse when the currents are alternately doubled and halved by taking out and inserting resistance in circuit, and since even with a slight delay the stronger current burns away the carbon points very rapidly, and so makes the distance between them for the stronger currents greater than for the weaker, it was thought better in the later experiments merely to take readings of the voltmeter when the resistance was altered backwards and forwards sufficiently to alternately treble and diminish to one third the current as shown by the earlier experiments. The following are samples of the readings of the voltmeter, the distance between the carbons in each case being fixed and the current alternately trebled and diminished to one third.

Current approximately.	Difference of potentials between carbons, in volts.	
1	26·5	
3	26·5	
1	24·5	
3	25·5	
1	26·5	Distance between carbons re-adjusted.
3	26·5	
1	25·5	
3	26·5	
3	32	A somewhat greater distance between the carbons.
1	30	
3	39·5	
1	30	
3	34	
1	34	
3	36	
1	38	
3	30	
1	28	
3	30	
3	30	Distance between carbons re-adjusted.
1	28	
3	30	
1	28	
3	30	

It would appear therefore that for a fixed distance between the carbons the difference of potentials necessary to maintain the arc is nearly but not quite independent of the current, the electromotive force requiring to be slightly increased when the current is very much increased.

2. The second part of the investigation was for the purpose of ascertaining in what way the differences of potentials between the carbons varied with the length of the arc when the current was kept constant. For this purpose the arc was projected on a distant scale by means of a lens, the magnifying-power of the arrangement being calculated, first, by comparing the distance between the scale and the lens with the distance between the arc and the lens, secondly by putting close to the arc a piece of carbon of known thickness and measuring quickly the thickness of its image as projected on the distant screen, before the piece of carbon had time to burn.

For each set of experiments a particular current was decided on: the carbons were put successively at different distances apart and the resistance in the circuit varied until the prearranged current was flowing through the arc, when instantly the actual projected distance between the carbons on the screen was read off and the difference of potentials between the carbons in volts; or the resistance in the circuit external to the lamp could be left fixed, and the carbons gradually withdrawn until the prearranged current was flowing through the arc, when, as before, the projected length of arc and the difference of potentials between the carbons was read off. A large number of experiments were made in this way with a Brush machine for currents varying between 5·5 and 10·4 ampères, the distances between the carbon points from 0 to one and a quarter inch, and the difference of potential varying from 0 to 140 volts, the carbons being 0·24 inch thick. The result when plotted gave a curve similar to that shown in fig. 2 (p. 202), horizontal distances representing distances between the carbon points, and vertical distances the difference of potentials between the carbons. For all the currents approximately the same curve was obtained—a result to be expected, seeing that the first investigation showed that the difference of potentials between two carbons necessary to produce an arc depended almost entirely on the distance between them, and hardly at all on the strength of the current. The equation to the curve we find to be approximately as follows:—

$$E=63+55a-63\times 10^{-10a},$$

where E is the difference of potentials in volts between the carbons, and a the distance between their points in inches. It will be seen that at first the difference of potentials necessary to maintain the arc increases rapidly with the distance, and that at a distance of about one tenth of an inch it is about 60 volts. From this the curve bends rapidly up to a point corresponding with a distance between the carbons of about one quarter of an inch; and for greater distances between the carbons than one quarter of an inch, the increase of difference of potentials becomes nearly proportional to the increase of distance, being about 54 volts per inch increase.

This law is very like that found by Mr. C. F. Varley for the discharge through a vacuum-tube, which was that the

current was proportional to the difference of potentials minus a constant; for this is equivalent to saying that, *cæteris paribus*, the difference of potentials necessary to produce a fixed current is proportional to the length of the tube plus a constant. The curve we have obtained is also strikingly like that obtained by Drs. W. De La Rue and Hugo Müller for the connexion between the electromotive force and the distance across which it would send a spark*. These gentlemen also made experiments on the electric arc with their large battery; but we do not find recorded any results with carbon points. On page 185 of the reprint from the 'Philosophical Transactions' of the account of their researches, the result of an experiment in air between two brass points is given; but according to that, when the arc was half an inch in length the difference of potentials between the brass points was that of 657 of their cells, or about 700 volts. How far the very high electromotive force found by Drs. W. De La Rue and Hugo Müller, to be necessary in this case, arose from a combination of the material employed for the electrodes and the smallness of the diameter of the brass electrodes, or whether the law that "the electromotive force necessary to maintain an arc depends mainly on the length of the arc and hardly at all on the strength of the current" fails when the current is below a certain small limit, we are unable to say; but of course both the diameter of the brass electrodes they employed and the strength of the current that was passing (0·025 ampère) in the arc was very much less than that used in any ordinary electric light, and to which the experiments of Mr. Schwendler and ourselves especially refer. It is very probable that the difference in the material of the electrodes has mainly to do with the difference between their results and ours; and we think it very probable that with very soft carbons an arc of a given length could be maintained with a much less difference of potentials than that found by us, since it would be more easy for a shower of carbon particles to be maintained between the ends of the carbons.

We have used as the title of this short communication the Resistance of the Electric Arc; but we are perfectly aware of

* Page 32 of the Reprint of "Experimental Researches on the Electric Discharge," Phil. Trans. part i. vol. 169.

the objections to this expression. How far the opposition to the passage of the currents in an electric arc is due to pure resistance, and how far to an opposing electromotive force, is up to the present time by no means certain. That there is some opposing electromotive force, seeing that mechanical disintegration of the carbon and transporting of its particles occurs, is, as was pointed out some years ago by Edlund, almost certain; but seeing that this opposing electromotive force ceases to exist with the extinction of the arc, and probably varies as the pure resistance varies, and further remembering that an opposing electromotive force which has no existence apart from a combined resistance acts in any electrical test exactly as a resistance, it must be always very difficult experimentally to separate them. All of course that we can measure electrically is the difference of potential between the carbons and the current passing between them; and this is what we have been measuring all through these two investigations.

It may be here noted that in all probability the conduction from particle to particle in a microphone is of the nature of a small electric arc, or, rather, perhaps a convective discharge, seeing that the resistance in a microphone varies with the current used to measure it; indeed it is probable, when the pieces of carbon or other material employed, are so pressed together that close intimacy of contact of the particles makes the resistance tolerably independent of the current, that then the pieces of carbon will not act as a microphone at all.

We have to thank Messrs. W. Atkinson and Lt. B. Atkinson, two of our students, for much assistance rendered us in these experiments.

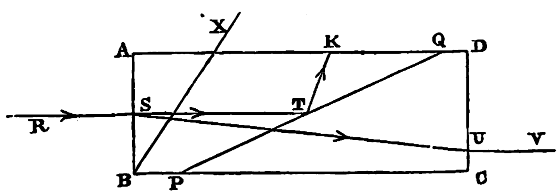
XXIV. *On Polarizing Prisms.* *By* R. T. GLAZEBROOK, *M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator in the Cavendish Laboratory, Cambridge* *.

IN a paper on Nicol's prism (Phil. Mag. vol. x. 1880) I have considered some of the defects of Nicol's prism as a means of producing plane-polarized light. In the present

* Read April 14, 1883.

paper I propose to describe a form of polarizing prism free from most of these. For many purposes, one of the great objections to Nicol's prism is the lateral displacement produced by it in the image of an object viewed through it. If we place a Nicol before the object-glass of a telescope, on turning the Nicol round its axis the image moves across the field. This has been remedied somewhat by cutting prisms with their ends at right angles to their length, and making the angle between the normal to the face on which the incident light falls and the plane of Canada balsam such that the ordinary ray is totally reflected there while the extraordinary ray passes through. But this is not entirely successful; for let A B C D (fig. 1) be a section of such a prism by a plane parallel to the edge B C and at right angles to the Canada balsam. Let P Q be the trace of the balsam. In an ordinary Nicol's prism A B would be inclined at about 74° to A D, and P Q

Fig. 1.



would be at right angles to A B, A D and B C being parallel to edges of the rhomb of spar. In the case now being considered, A B is at right angles to B C, B C being still parallel to a rhombic edge.

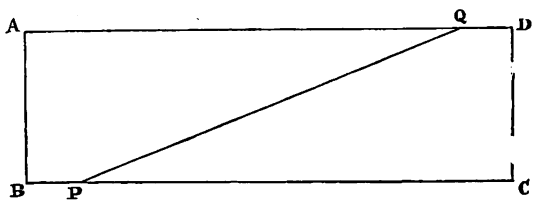
Consider a ray R S incident normally on A B. The ordinary ray S T enters the spar without deviation, but is reflected by the balsam at T in direction T K; the extraordinary ray is refracted at the face A B in direction T U, and turned from its original path in virtue of the extraordinary refraction. It emerges along U V parallel to its original direction, but displaced to one side, so that the extraordinary image of the object seen is displaced to one side by the passage of the light through the spar.

In the prism considered in fig. 1, the optic axis lies in the plane of the paper, making an angle of 57° 30′ with B C.

Suppose now we cut a rectangular parallelepipedon from a piece of spar, in such a way that two of its faces are at right angles to the optic axis while the other four are parallel to it.

Let A B C D (fig. 2) be a section of the solid by a plane also at right angles to the optic axis, and therefore parallel to two faces and at right angles to the other four; and suppose that

Fig. 2.

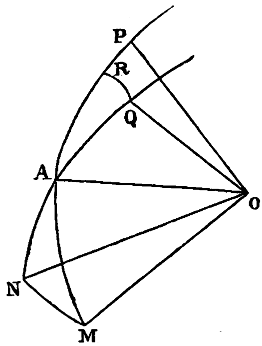


B C is about three times A B. Let P Q be inclined at about 20° to B C, and suppose the prism cut in two by a plane at right angles to the paper and passing through to P Q. Then let the faces of section be polished, and cemented together with Canada basam. The optic axis will be at right angles to the plane of the paper, and the section of the wave-surface by that plane will be two circles of radii A and C, these being the ordinary and extraordinary wave-velocities respectively. Hence a ray falling on the face A B in any direction in the plane of the paper will be divided into two, which will both undergo ordinary refraction, so that if the incident ray be normal to the face A B, the extraordinary and ordinary rays in the prism will coincide in direction, both being normal to the same face. The extraordinary ray is not deviated by the refraction; so that no lateral displacement of the extraordinary image is produced by the prism. The ordinary ray is incident at about 70° on the face P Q; it is therefore totally reflected, and the emergent light is plane-polarized. The prism differs from one described by Prof. S. P. Thompson (Phil. Mag. Nov. 1881) only in the fact that its ends are normal to its length instead of being inclined obliquely to it. But this form of prism has other and more important advantages.

Let O M, O N (fig. 3) be two extraordinary wave-normals

and O A the optic axis. Pass a plane M O A through O M and O A, and in this plane draw O P at right angle to O M; then O P is the direction of vibration in the wave which travels along O M. Similarly, if N O A be a plane through O N and O A, and O Q a line in it at right angles to O N, O Q is the direction of vibration for the wave along O N; and it may happen, clearly, that O P and O Q are inclined to one another at a large angle even when O M and O N are close together. Suppose, then, that the extraordinary pencil of wave-normals which is traversing the spar is slightly conical, and that O N, O M are two of the wave-normals; the planes of polarization are inclined to each other at an angle equal to P O Q; and this may be considerable. Or, again, suppose that we have a polarized pencil of parallel wave-normals incident on the prism. We determine the position of their plane of polarization by turning the prism until no light passes through. Suppose that, when this is the case, the incident light is parallel to O M. Now let the plane of polarization of the incident light be rotated, and suppose we wish to measure this rotation; we turn the prism until the light is again quenched. Theoretically the axis round which the prism has been turned should be parallel to O M. In practice it is difficult to ensure this; and in general the direction of the wave-normal relatively to the optic axis will be changed, and may now be O N say. But since the planes of polarization of the waves along O M and O N are different, the angle through which the prism has been turned will not be the angle through which the plane of polarization of the incident light has moved.

Fig. 3.



Now Nicol's prism is so cut that the angle between the planes of polarization of two waves inclined to each other at but a small angle as they traverse the crystal is considerable. If, then, a slightly conical pencil traverse the prism, the angles between the planes of polarization of the different waves are considerable; or if a parallel pencil traverse the prism inclined

at but a small angle to the axis of rotation, and the plane of polarization of this beam be rotated, that rotation will differ considerably from the angle through which the prism has to be turned to reestablish blackness.

In our figure the wave along O M is polarized in a plane at right angles to O P, that along O N in a plane at right angles to O Q. Consider now a conical pencil of wave-normals in air: it is clearly impossible for it to be plane-polarized, if by plane polarization we mean that the directions of vibration are parallel to the same line; for we cannot have a series of lines touching a sphere all parallel to the same line. Such a pencil, however, may be said to be most nearly plane-polarized when all the directions of vibration are parallel to the same plane; and this plane will be that which passes through the axis of the pencil and the direction of vibration for the wave-normal which coincides with the axis. For if this be the case, the whole of the pencil can pass unaltered either as an ordinary or extraordinary wave through a piece of spar on which its axis falls normally, provided that the optic axis of the spar be respectively either at right angles to or parallel to the plane in question. Using "plane polarization" in this sense, we proceed to consider when a conical pencil of given vertical angle travelling in a piece of uniaxal crystal is most nearly plane-polarized.

Now let O M (fig. 3) be the axis of the pencil, and O P the direction of vibration for the light travelling along O M, and let O N be any other wave-normal. According to the above statement, the conical pencil will be most nearly plane-polarized if the vibration travelling along O N is parallel to the plane P O M. If, however, the pencil be travelling in a crystal, it is clearly impossible in general for the displacement along O N to be parallel to this plane. For let O A be the optic axis; O A lies in the plane M O P. Pass a plane through O A, O N, and in it draw O Q at right angles to O N; O Q is the direction of displacement which travels along O N, and O Q is not parallel to the plane P O M.

We can resolve the displacement along O Q into two, in and perpendicular to the plane P O M. The light then will be most nearly plane-polarized when the average intensity of the

vibrations normal to this plane is least; and it remains to find the condition for this.

In fig. 3 let QR be perpendicular to AP. Let AM$=\alpha$, NM$=\beta$, AN$=\theta$, AMN$=\phi$. Let ρ be the amplitude of the displacement along OQ. The displacement normal to the plane PAM is $\rho \sin$ QR, and the intensity of the wave is proportional to $\rho^2 \sin^2$ QR.

We are to consider a hollow conical pencil with OM as axis. An element of such a pencil at N will be $\sin\beta d\phi$; and the total energy in the pencil, so far as it depends on the displacement normal to the plane PAM, is

$$2\int_0^\pi \rho^2 \sin\beta \sin^2 \mathrm{QR}\, d\phi.$$

Now

$$\mathrm{AQ}=\frac{\pi}{2}-\theta,$$

$$\sin \mathrm{QK}=\sin \mathrm{AQ} \sin \mathrm{RAQ}$$

$$=\cos\theta \sin \mathrm{NAM},$$

$$\sin \mathrm{NAM}=\frac{\sin\phi \sin\beta}{\sin\theta};$$

$$\therefore \sin \mathrm{QR}=\cot\theta \sin\phi \sin\beta. \quad \ldots \quad (1)$$

Also

$$\cos\theta=\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi. \quad \ldots \quad (2)$$

Substituting in the value of sin QR, we find for the energy required the expression

$$2\rho^2\sin^3\beta\int_0^\pi \frac{\sin^2\phi(\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi)^2 d\phi}{1-(\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi)^2}. \quad . \quad (3)$$

And we require to evaluate this integral.

Let

$$\cos\alpha\cos\beta=a, \quad \sin\alpha\sin\beta=b.$$

Then

$$\int_0^\pi \frac{\sin^2\phi\,(\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi)^2\, d\phi}{1-(\cos\alpha\cos\beta+\sin\alpha\sin\beta\cos\phi)^2}$$

$$=\int_0^\pi \frac{\sin^2\phi(a+b\cos\phi)^2\, d\phi}{1-(a+b\cos\phi)^2}$$

$$=\int_0^\pi \frac{\sin^2\phi}{1-(a+b\cos\phi)^2}\,d\phi-\int_0^\pi \sin^2\phi\, d\phi.$$

The first term

$$=\tfrac{1}{2}\int_0^\pi\left\{\frac{\sin^2\phi}{1-(a+b\cos\phi)}+\frac{\sin^2\phi}{1+(a+b\cos\phi)}\right\}d\phi.$$

But

$$\int_0^\pi \frac{\sin^2\phi\, d\phi}{c+d\cos\phi} = \int_0^\pi \frac{(1-\cos^2\phi)\, d\phi}{c+d\cos\phi}$$

$$= \int_0^\pi \left\{ \frac{c}{d^2} - \frac{\cos\phi}{d} - \frac{c^2-d^2}{d^2(c+d\cos\phi)} \right\} d\phi$$

$$= \frac{\pi c}{d^2} - \frac{c^2-d^2}{d^2} \frac{2}{\sqrt{c^2-d^2}} \frac{\pi}{2},$$

if c is $> d$,

$$= \frac{\pi}{d^2} \{c - \sqrt{c^2-d^2}\}.$$

Hence

$$\int_0^\pi \frac{\sin^2\phi}{1-a-b\cos\phi}\, d\phi = \frac{\pi}{b^2}\{1-a-\sqrt{(1-2a+a^2-b^2)}\}; \quad (4)$$

for we can easily show that c is $> d$ in this case. And

$$\int_0^\pi \frac{\sin^2\phi}{1+a+b\cos\phi}\, d\phi = \frac{\pi}{b^2}\{1+a-\sqrt{(1+2a+a^2-b^2)}\}. \quad (5)$$

And the required integral is

$$\frac{\pi}{2b^2}\{2-\sqrt{(1-2a+a^2-b^2)}-\sqrt{(1+2a+a^2-b^2)}-b^2\}. \quad . \quad (6)$$

But

$$a^2-b^2 = \cos^2\alpha\cos^2\beta - \sin^2\alpha\sin^2\beta = \cos^2\alpha + \cos^2\beta - 1.$$

Hence, since the positive sign is to be attached to the roots, we have, if β be $< \alpha$,

Intensity required

$$= \frac{\pi\rho^2\sin\beta}{\sin^2\alpha}\{2-(\cos\beta-\cos\alpha)-(\cos\beta+\cos\alpha)-\sin^2\beta\sin^2\alpha\}$$

$$= \pi\rho^2\sin\beta(1-\cos\beta)\left\{\frac{2}{\sin^2\alpha}-(1+\cos\beta)\right\}$$

$$= 4\pi\rho^2\sin\beta\sin^2\frac{\beta}{2}\left\{\operatorname{cosec}^2\alpha - \cos^2\frac{\beta}{2}\right\}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

And if α be $< \beta$,

Intensity

$$= \pi\rho^2\sin\beta\left\{\frac{2}{1+\cos\alpha}-\sin^2\beta\right\} = \pi\rho^2\sin\beta\left\{\sec^2\frac{\alpha}{2}-\sin^2\beta\right\}. \quad (8)$$

In the first case the intensity is clearly least when α is $\frac{\pi}{2}$, its value then being

$$\pi\rho^2 \sin\beta(1-\cos\beta)^2;$$

and in the second case it is least when α is 0, and its value is

$$\pi\rho^2 \sin\beta \cos^2\beta.$$

This second minimum will be greater than the other if

$$\cos\beta \text{ is } > 1-\cos\beta,$$

i. e. if $\cos\beta$ is $> \frac{1}{2}$,

i. e. if β is $< 60°$.

If, then, a conical pencil whose semi- vertical angle is less than 60° be passing through the spar, the pencil will be most nearly plane-polarized if the axis of the pencil is at right angles to that of the spar.

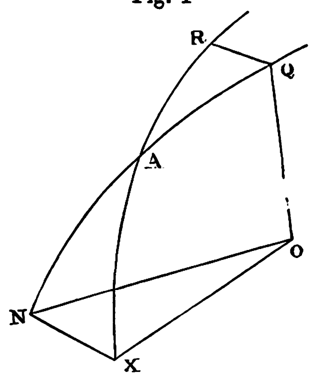
Now if the axis of a conical pencil pass normally through a prism cut as already described, it will be at right angles to the optic axis; and hence the pencil, if its semi- vertical angle be less than 60°, will be more nearly plane-polarized than it would be if the axis occupied any other position. This constitutes a second advantage in favour of the new prism.

Again, suppose we have a parallel pencil of wave-normals in direction ON, and that the axis round which the prism rotates is OX (fig. 4). In our observations we suppose that these two coincide, and work as if the plane of polarization of the emergent light coincided with that of light travelling along OX, thus introducing an error. The amount of this error will depend of course partly on the angle NX (β say), and partly on the angle NXA (ϕ say), OA being the optic axis. If we know β and ϕ we can calculate the error, and could determine the value to be given to XA or α to make it the least possible.

But in practice ϕ may be anything between 0 and 2π, and β anything between 0 and a not very large angle β_1; and the question arises, what value must we assign to α in order that the error produced by any chance values of β and ϕ may most probably be as small as possible? To answer this we require to determine, between these limits for β and ϕ, the

mean of the values, irrespective of sign, of the amplitudes of the vibrations normal to the plane OAX.

Fig. 4



Let O Q be the direction of vibration for the wave-normal O N, and Q R perpendicular on the plane A X. Then the displacement normal to the plane AOX is $\rho \sin QR$; and, with the same notation as before, the average displacement

$$=\frac{\rho}{2\pi}\int \sin\beta \sin\phi \cot\theta \, d\phi. \quad . \quad . \quad . \quad . \quad (9)$$

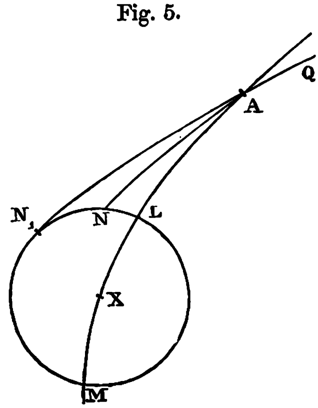
Also $\sin\theta \, d\theta = \sin\alpha \sin\beta \sin\phi \, d\phi$.

Hence average displacement

$$=\frac{\rho}{2\pi \sin\alpha}\int \cos\theta \, d\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

We must consider now the limits between which the integral is to be taken.

Fig. 5.



Describe a circle (fig. 5) passing through N with X as centre, and cutting A X in L and M, and if possible let N_1 be a point on this circle such that

$$N_1A = \frac{\pi}{2}.$$

Suppose we treat displacements to the right of AX as positive. When N coincides with L,

$$\theta = \alpha - \beta.$$

If N lies between L and N_1, Q is to the right of AX, as in figure, and the displacement is positive. We must therefore integrate for θ from $\alpha - \beta$ to $\frac{\pi}{2}$. But if N lie between N_1 and M, Q will be to the left of AX, and the displacement will be negative. Thus, to get the whole average displacement, irrespective of sign, we have to subtract the value of the integral

from $\frac{\pi}{2}$ to $\alpha+\beta$. If, however, $\alpha+\beta$ is $< \frac{\pi}{2}$, no position such as N_1 can be found, and we have to integrate straight from $\alpha-\beta$ to $\alpha+\beta$. The same is true for positions of N on the other side of AX.

Hence, in the first case, the average displacement normal to the plane is

$$\frac{2\rho}{2\pi \sin\alpha}\left\{\int_{\alpha-\beta}^{\frac{\pi}{2}} \cos\theta\, d\theta - \int_{\frac{\pi}{2}}^{\alpha+\beta} \cos\theta\, d\theta\right\}$$

$$= \frac{\rho}{\pi \sin\alpha}\{1-\sin(\alpha-\beta)+1-\sin(\alpha+\beta)\}$$

$$= \frac{2\rho}{\pi}\left\{\frac{1}{\sin\alpha} - \cos\beta\right\}$$

$$= \frac{2\rho}{\pi}\{\operatorname{cosec}\alpha - \cos\beta\}. \quad \ldots\ldots \quad (11)$$

And in the second case it is

$$\frac{2\rho}{2\pi \sin\alpha}\int_{\alpha-\beta}^{\alpha+\beta} \cos\theta\, d\theta = \frac{\rho}{\pi\sin\alpha}\{\sin(\alpha+\beta)-\sin(\alpha-\beta)\}$$

$$= \frac{2\rho}{\pi}\cot\alpha \sin\beta. \quad \ldots\ldots \quad (12)$$

The first is clearly least when α is $\frac{\pi}{2}$; the second decreases as α increases, but has no minimum; for after a time we should reach a point at which $\alpha+\beta$ became equal to $\frac{\pi}{2}$, and then the limits would require changing: for this value, of course, the two integrals are the same.

Thus the average displacement normal to the plane OAX is least when OX is at right angles to the optic axis, and hence the average error in the position of the plane of polarization is least also. The average displacement just calculated is of course that for a given value of β. If we require the average for any value of β between 0 and β_1, we must multiply our expressions (11) and (12) by $d\beta$, and, integrating from 0 to β_1, divide the result by β_1.

To show the difference in this respect between the new prism and Nicol's, let us calculate the displacement normal to the plane AOX in the two cases, supposing the value of β is 5°.

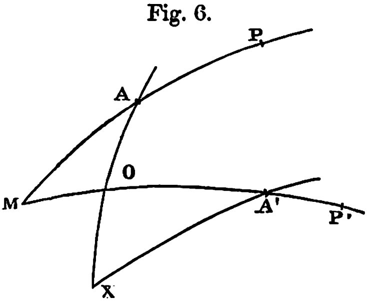
In Nicol's prism, $\alpha = 63° \ 30'$; so that $\alpha + \beta$ is less than $\frac{\pi}{2}$, and the second formula (12) must be taken. The ratio of the two displacements is therefore

$$\frac{\cot 63° \ 30' \sin\beta}{1-\cos\beta} = \cot 63° \ 30' \cot\frac{\beta}{2};$$

and substituting the value $\beta = 5°$, this comes to 434 : 39, or about 11 to 1. Thus the average error in the position of the plane of polarization as determined by the new prism will be about one eleventh of that which would be produced by the same errors of adjustment with a Nicol's prism; while the amount of light polarized out of the proper plane will be less than one per cent. of that which would be produced by a Nicol.

Again, suppose the prism is turned through an angle ω about OX (fig. 6), and let us inquire what is the angle through which the plane of polarization of the emergent light is rotated. Let O A′ be the new position of the optic axis. Join M A, M A′, and in them take points P, P′ such that $MP = MP' = \frac{\pi}{2}$. O P, O P′ are the directions of vibration for the waves travelling along O M in the two positions of the prism respectively. The angle through which the plane of polarization has been turned is P P′ or P M P′, that through which the prism has been turned is A X A′; and we require to investigate the conditions under which the average difference between these two for all possible positions of M within a certain distance, β_1 say, of X.

Fig. 6.



Now we have seen already that if the axis of rotation be at right angles to the optic axis, the average error produced in the determination of the position of the plane of polarization for each of the two positions of the prism will be a minimum; and hence it follows that the average error in the angle between these two positions is a minimum also.

All these results, of course, hold only for the position of the

plane of polarization of the light when in the crystal, and will be modified by the refraction that takes place as the waves emerge into the air. But since the ends of the prism are normal to its length, for all the waves considered the incidence is very nearly direct, and the change produced by refraction in the position of the plane of polarization is very small indeed.

Thus a prism cut as described possesses the following advantages over Nicol's prism:—

1. There is no lateral dispacement in the apparent position of an object viewed through it.

2. A conical pencil whose axis passes directly through is more nearly plane-polarized than would be the case if the axis of the prism were related to that of the spar in any other manner.

3. If the direction of the wave-normal within the prism does not quite coincide with the axis of rotation, the average error in the position of the plane of polarization is less than for any other method of cutting.

I hope shortly to have some prisms cut by Mr. Hilger in this manner, and to test by means of them the theoretical conclusions arrived at in the paper.

Note added April 26th.

If the plane of section PQ be inclined to BC at an angle of 20°, as in fig. 2, the angular aperture of the field will be small, only about 10°, and it will be necessary that all the light traversing the prism should be very nearly parallel to BC. The aperture may be increased up to about 20° by lengthening the prism considerably and decreasing the angle between PQ and BC. If this be reduced to 11°, the aperture will have its maximum value of 22°.

The aperture may be somewhat increased, and the length of the prism shortened, by using as the separating medium balsam of copaiba, as was suggested at the meeting of the Physical Society at which this paper was read.

The mean index of refraction for this substance is about 1·52, as determined by Brewster. The angle of total reflexion therefore for the ordinary ray is $\sin^{-1}(1{\cdot}52/1{\cdot}66)$, or about 66°, while for Canada balsam this angle is about 68°. The

possible aperture, using the balsam of copaiba, thus is about 24°.

Professor Thomson's prism, mentioned already, will have a wider field. But it must be remembered that the new prism was not designed for microscopic work, but to obviate the displacement in the image referred to at the commencement of the paper, and to produce a field in which the plane-polarization should be as nearly as possible complete.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

May 1883.

XXV. *Colour-Sensation.* *By* H. R. Droop, *M.A.**

The generally received theory of colour-sensation is that there are three colour-senses in the eye, and that all the different impressions of colour received by the brain are due to those three colour-senses being affected in different proportions by the light entering the eye. This theory was originally propounded by Young, and was revived by Helmholtz and Maxwell, who established experimentally certain laws of colour-sensation, from which laws the theory of three colour-sensations was a probable, but (as I shall show further on) not a necessary, inference.

Maxwell proved experimentally† that a linear equation of the form

$$X = vV + cC + uU$$

could always be found, expressing any colour and shade of colour perceived by the normally constituted eye in the terms of any three given colours (whether pigments—*e. g.* vermilion, chrome yellow, and ultramarine—or selected rays of the spectrum) as seen by the same eye. This equation, when the

* Read April 28, 1883.

† See Transactions of the Royal Society of Edinburgh, vol. xxi.; Philosophical Transactions, 1860, p. 57.

coefficients v, c, and u are all positive (*i. e.* when the three given colours are sufficiently intense and distinct from each other), means that the colour X could be produced by combining together (*i. e.* presenting to the eye simultaneously) certain proportions of the three given colours. From this it obviously followed that every such colour and shade *could* be produced by three colour-sensations, each of which, when excited, conveyed to the brain the impression of a homogeneous colour.

Helmholtz arrived at the same conclusion by proving (see *Handbuch der physiologischen Optik*, p. 282, ed. 1867) that any given colour could be produced by combining a certain quantity of white with some particular colour of the spectrum. From which he deduced that every colour and shade of colour depended on only three independent variables, viz. the quantity of the spectrum-colour, the quantity of white, and the length of wave of the spectrum-colour. But though the theory of three colour-sensations was the simplest and most obvious explanation of the experimental facts thus established, it is not (as has been commonly assumed) the only theory capable of explaining them. They would be equally well explained by supposing that there are four, five, or more colour-sensations connected by a sufficient number of linear equations of condition to reduce the number of independent variables to three. Obviously this will satisfy all that Helmholtz established. That it will also explain the law established by Maxwell may be shown as follows.

Suppose that there are four colour-sensations, R, Y, G, and B (the red, yellow, green, and blue sensations), and that each of them is expressed as a linear function of the three standard colours, V, C, and U, as every colour seen by a normal human eye can be expressed.

Then we shall have four equations of the form

$$\left.\begin{aligned} R &= v_r V + c_r C + u_r U, \\ Y &= v_y V + c_y C + u_y U, \\ G &= v_g V + c_g C + u_g U, \\ B &= v_b V + c_b C + u_b U. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (A)$$

And when we eliminate V, C, and U between these four equations we shall get a linear relation between R, Y, G, and B, which is the condition that these four colour-sensations

should be capable of being expressed as linear functions of V, C, and U, *i. e.* should be colour-sensations coexisting in a normal eye.

If we supposed five colour-sensations, we should have five equations (A) between which to eliminate V, C, and U, and should get two linear equations between the five colour-sensations.

The proposition that four colour-sensations with a linear relation between them will satisfy Maxwell's law may also be tested in another way, viz. by assuming that there are four colour-sensations connected by a linear equation

$$rR+yY+gG+bB=0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and that V, C, and U, the three standard colours, are known to result from these colour-sensations being effected in certain proportions.

E. g. V is known to result from R, the red colour-sensation, being effected to an extent r_v, Y to an extent y_v, G to an extent g_v, and B to an extent b_v; and may therefore be represented by the equation

$$V=r_vR+y_vY+g_vG+b_vB; \quad . \quad . \quad . \quad . \quad (2)$$

and similarly C and U may be represented by

$$C=r_cR+y_cY+g_cG+b_cB, \quad . \quad . \quad . \quad . \quad (3)$$

$$U=r_uR+y_uY+g_uG+b_uB. \quad . \quad . \quad . \quad . \quad (4)$$

Then from these four equations we can express R, Y, G, and B as linear functions of V, C, U. But every possible colour X must be produced by exciting all or some of the four colour-sensations, and therefore must be capable of being expressed by an equation

$$X=r_xR+y_xY+g_xG+b_xB.$$

Consequently, if R, Y, G, and B be replaced in this equation by the linear functions of V, C, and U which represent them, every such colour X can be expressed as a linear function of V, C, and U; *i. e.* every such colour will conform to the law, which Maxwell and Helmholtz established, of being capable of being made up of any three standard colours.

The same reasoning might obviously be applied in like manner to five colour-sensations connected by two linear equations of condition, or to n colour-sensations connected by $n-3$ linear equations of condition.

I have taken up this question and endeavoured to show that what Maxwell and Helmholtz established is not inconsistent with the existence of four or more colour-sensations (provided certain relations exist between them), because a certain recent discovery seems to me to have given a particular hypothesis, involving four colour-sensations, a strong claim to be accepted as, in the main, true. This discovery relates to the colours actually seen by colour-blind persons. Two persons have been discovered who, being each colour-blind of only one eye, can explain how far the colours seen by their colour-blind eyes agree with, or differ from, those seen by their normal eyes. It has been found that each of these persons has two colour-sensations complementary to each other. One sees yellow and blue, and is blind to red and green; while the other sees red and bluish green, and is blind to blue and yellow; and with each of them the combination of his two colour-sensations in proper proportions produces white or grey. Professor Holmgren, of Upsala, has given an account of both these cases in the 'Proceedings of the Royal Society,' vol. xxxi. p. 302; and Professor Hippel, of Giessen, has given an account (differing in some respects) of the first or blue-yellow case in Gräf's *Archiv für Ophthalmologie*, vol. xxvi. p. 176, vol. xxvii. pt. 3, p. 47.

These two cases suggested to Professor Preyer of Jena a theory (which he propounded in 1881 in Pflüger's *Archiv*, vol. xxv.) that ordinary eyes have two pairs of colour-sensations—(1) yellow and blue, and (2) red and bluish green, and that the colour-blindness which consists in confusing red and green, or, as the case may be, blue and yellow, is due to the absence of one pair of these sensations. But Professor Preyer does not deal with the difficulty that Helmholtz and Maxwell are supposed to have proved, that there cannot be more than three colour-sensations, although that view is treated as unquestionable by other recent writers, *e. g.* by Professor Donders (Gräf's *Archiv für Ophthalmologie*, vol. xxvii.), and by Professor v. Kries, of Freiburg im Breisgau (*Die Gesichtsempfindungen und ihre Analyse*, Leipzig 1882, p. 33); and it is naturally a serious obstacle to the fair consideration of Professor Preyer's theory.

But inasmuch as Professor Preyer supposes that in each pair of his colour-sensations the one sensation is complemen-

tary to the other, we have the equation

$$R+G=\text{White}=Y+B,$$

a linear relation between the four colour-sensations; and therefore it follows, from what I have already proved, that this hypothesis of two pairs of complementary colour-sensations is quite consistent with what Maxwell and Helmholtz established.

This theory of Professor Preyer's explains the leading facts of colour-blindness, viz. that a colour-blind eye only perceives two homogeneous colours, and that it is unable to distinguish between red and green, or, as the case may be, between blue and yellow. It is impossible to ascertain with absolute certainty that persons who are colour-blind with both eyes see the same colours as the two persons who have been discovered colour-blind of only one eye; but it is noteworthy that when Dr. Pole made that minute examination of his colour-blindness, the results of which he gave in the 'Philosophical Transactions' for 1859, he came to the conclusion that the colours he saw were yellow and blue and, as the result of their mixture, white; and he only gave up this view in deference to the three-sensation theory then supposed to be conclusively established.

The following facts, not connected with colour-blindness, seem to me to give considerable support to the hypothesis of two pairs of complementary colour-sensations:—

(1) Observations have been made as to the sensibility of different parts of the retina to different colours, and also as to the effect of diminishing the angles subtended at the eye by small coloured objects; and in both cases red and green colours are found to comport themselves alike, and differently from blue and yellow. When an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than sensibility to red and green; while, on the other hand, if the angular magnitude of the object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow (von Kries, *Gesichtsempfindungen*, pp. 93, 95).

(2) In cases where the colour-senses become affected by disease of the eye, the order in which different colours are

found to disappear agrees with the theory of four colour-sensations. In cases of atrophy of the optic nerve, it seems pretty clearly established that green becomes invisible first, then red, then yellow, while the perception of blue remains the longest (see Leber, *Archiv für Ophthalmologie*, vol. xv.; Leber, *Handbuch der Augenheilkunde*, vol. v. p. 1039; Schön, *Lehre vom Gesichtsfelde und seine Anomalien*).

In cases where the sight is affected by excess in alcohol or tobacco, Nuel found that green and red became invisible simultaneously, and blue and yellow later (*Annales de l'Oculiste*, 80, p. 110, as cited in von Kries, *Gesichtsempfindungen*, p. 156).

On the other hand, the received hypothesis of three colour-sensations does not readily explain how it is that one colour-blind eye sees blue, yellow, and white, and another red, green, and white, as Professors Holmgren and Hippel have found to be the case. If there are only three colour-sensations, it is generally agreed that they must be red, green, and violet. If one of these three sensations were wanting, we should naturally expect that the defective eye would have the other two sensations of a normal eye. For instance, if the red sensation were missing, we should expect that the eye would see green and violet, and white would appear of a bluish green complementary to the missing red. Similarly if green were missing, we should expect that the defective eye would see red and violet; while if violet were missing, it would see red and green, and white would appear yellowish complementary to violet.

The only attempted explanation* I have seen of this proceeds from Professor Donders, who suggests that where one of the three colour-sensations has been wanting from birth, its absence may have modified the development of the other two sensations. He says:—"The retina is not an instrument with three strings of which one has suddenly snapped. It is a living instrument whose three differently toned strings have been developed in conjunction with each other" (Gräf's *Archiv für Ophthalmologie*, vol. xxvii. p. 212).

* Professor Holmgren, when communicating the two cases of one-sided colour-blindness to the Royal Society, states that he considers these phenomena quite consistent with the theory of three colour-sensations; but he does not explain how he reconciles them with it, except by referring to works which I have not been able to get access to.

This explanation is ingenious; but it assumes that all cases of colour-blindness to red and green are from birth, whereas, though this is usually so, there are several alleged cases of acquired colour-blindness to red and green. Dr. Joy Jeffries, pp. 50–52, gives two cases, one discovered by Professor Tyndall, the other by Mr. Haynes Walton; and M. Nuel has also described one, *Annales de l'Oculiste*, 80, 82, as quoted by v. Kries, *Gesichtsempfindungen*, p. 154.

Moreover Professor Cohn (*Deutsche medicinische Wochenschrift*, 1880, No. 16, cited by von Kries, p. 158) claims to have temporarily restored to normal colour-vision a person affected with red-green colour-blindness from birth. If this be so, there cannot well have been any such abnormal development of his other colour-sensations as Professor Donders supposes. Such a development could hardly have been suddenly cured by artificial means.

Those colour-blind persons who cannot distinguish between red and green have been divided into two classes, according as they can perceive rays towards the end of the spectrum or are unable to do so. According to many adherents of the theory of three colour-sensations, those who can perceive rays at the red end want the green-colour sense, while those who cannot do so want the red sense. It seems to be established that rays at the green part of the spectrum do not make as much impression on the so-called green-blind as on the so-called red-blind; and Professor Donders has lately ascertained by careful measurements with two cases, one of so-called red-blindness, and the other of so-called green-blindness, that throughout the red and orange parts of the spectrum the red-blind eye perceives less light than the green-blind eye, and that the opposite is the case in the green portion of the spectrum (Gräf's *Archiv für Ophthalmologie*, vol. xxvii.; Transactions of the International Medical Congress for 1881, vol. i.p. 277).

But there seem to me to be several serious objections to explaining the difference between so-called green-blind and so-called red-blind by the hypothesis of three colour-sensations:—

(A) If there are only three colour-senses, the green sense must be of a yellowish green capable of producing yellow and orange when combined with the red sense, and very different from the bluish green which is complementary to red. There-

fore if one class of colour-blind persons have lost the red sense and the other the green sense, there ought to be considerable differences in all the colour equations obtained from the two classes of eyes, and especially in the proportions of blue and yellow which will neutralize each other. But I have not met with any trace of such differences having attracted attention, except in equations between red and green, and it is clear that in the spectrum the neutral point where the blue or violet colour-sense neutralizes the other colour-sense is very nearly the same for red-blind and green-blind persons. Professor Donders fixes it for his red-blind case at a wave-length of 494·85 millionths of a millimetre, and for his green-blind case at 502·3 millionths, the difference 7·5 being not one fiftieth part of the difference between the greatest and least wave-lengths in the visible spectrum; while Professor Preyer, in another case, found that doubling the amount of light altered the neutral point from 512·8 to 506·6, *i. e.* nearly as much (Pflüger's *Archiv*, vol. xxv.).

(B) If blindness to the red end of the spectrum were due to the absence of the red sense, it would be the same in extent in different red-blind persons, whereas in fact it differs considerably. (Donders, Gräf's *Archiv*, vol. xxvii.)

(C) The extent to which the violet end of the spectrum is obscured to violet- or blue-blind eyes also varies very much. Professor Stilling (*Klinische Monatsblätter für Augenheilkunde*, 1875, Beilage 2) met with three cases in which the green thallium-line between E and D formed the boundary of the visible spectrum; while in another case (*Centralblatt für praktische Augenheilkunde*, 1878, p. 99) the same observer found that nearly the whole of the spectral green was perceived, and grey or, in a faint spectrum, red beyond it. In the case of one-eyed violet- or blue-blindness described by Professor Holmgren ('Proceedings of the Royal Society,' vol. xxxii. p. 305) "the spectrum is continued over the place where we see green, greenish blue, cyan-blue, and indigo to the commencement of the violet, where it absolutely ended with a sharp limit about Fraunhofer's line G."

(D) As is well known, inability to distinguish between red and green has in many cases been found to exist among different members of the same family, and especially among

brothers; and therefore when such colour-blindness is found to exist among relations, there is a very strong probability that they have inherited the same affection of the eyes. Therefore if red-blindness and green-blindness be distinct things, due to the absence of different colour-senses, we should expec- that the colour-blind members of the same family would be either all red-blind or all green-blind. But this is not the case. Among the colour-blind cases examined by Professor Stilling (*Klinische Monatsblätter für Augenheilkünde*, 1875, Beilage 1 and 2) there were two pairs of brothers both unable to distinguish between red and green; and in each case one brother was able to perceive light at the red end of the spectrum, while the other was not.

All these reasons lead me to believe that the difference between so-called red-blind and so-called green-blind is not due to their having lost different colour-senses, but rather to the loss of one pair of colour-senses, those for red and green being, in the case of the so-called red-blind, accompanied by some disturbance of the other pair of colour-senses —a disturbance varying in character and degree in different cases, and similar to what is found to exist in different cases of blue- or violet-blindness.

The shapes of the curves Professor Donders has published (Trans. International Medical Congress, 1881, vol. i. p. 280) to represent the respective intensities of the light perceived by a red-blind and a green-blind eye have suggested to me a possible explanation of the difference between these two eyes. The curves representing the less-refrangible sensation of each eye correspond very nearly in shape and dimensions; but that for the red-blind eye is shifted some way further from the red end of the spectrum. On the other hand, the curves representing the more-refrangible sensation of the two eyes are almost identical in position as well as in shape and size. This effect would be produced if the organization producing the less-refrangible (or yellow) colour-sensation in the green-blind eye were so modified in the red-blind eye as to make shorter waves produce the same effects which longer waves produced in a green-blind eye. The change supposed would be equivalent to shifting the tone of a musical instrument an octave higher.

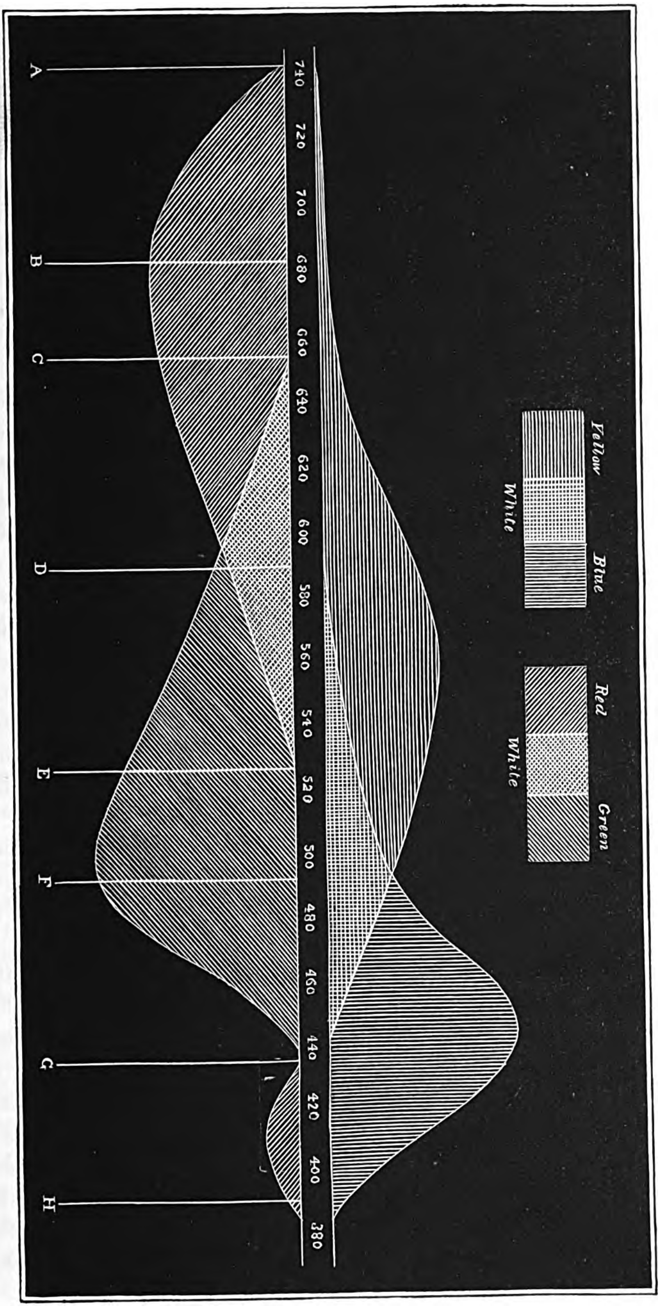
But I do not suppose that all cases of shortened spectrum could be thus accounted for.

In order to show more in detail what my views of colour-sensation are, I have prepared a diagram representing roughly how the two pairs of colour-senses are affected by the different rays of the spectrum. The upper half of the diagram represents the effects produced by the different rays on the yellow and blue colour-senses, and the lower part those they produce on the red and green colour-senses. The portion with crossed vertical and horizontal lines represents the extent to which the same rays operate on both the yellow and the blue colour-senses, and thus produce white light, which (combining with the colour produced by the colour-sense which is most affected) produces a whitish yellow or a whitish blue. Similarly the portion crossed by diagonal lines in the lower half of the diagram represents the extent to which the same rays operate on both the red and the green colour-senses.

I have represented the complementary colour-sensations as thus overlapping *, because Professors Preyer and Hippel have found that, for red-green blind eyes, the neutral point in the spectrum which appears white or grey varies in position according to the intensity of the light (Pflüger's *Archiv*, vol. xxv.; Gräf's *Archiv für Ophthalmologie*, vol. xxvii. pt. 3). Professor Preyer found that enlarging the aperture through which the light was admitted from ·250 millim. to ·370 millim. shifted the neutral point, where the spectrum appeared grey, from where the wave-length was 512·8 millionths of a millimetre to where it was 506·6 millionths. This is readily intelligible, if the rays in this part of the spectrum affect both the yellow and the blue colour-senses, while the intensity of the light alters the proportions in which they are respectively affected by it.

It will be observed that a narrow strip of yellow extends

* The extent to which the colour-sensations are represented as overlapping rests on conjecture. Observations on colour-blind persons, *i. e.* persons with only two colour-sensations, give readily the neutral points where the sensations counterbalance each other and produce white; but the extent to which they overlap could only be inferred from observing when the colours cease to have any admixture of white.


A
B
C
D
E
F
G
H
740
720
700
680
660
640
620
600
580
560
540
520
500
480
460
440
420
400
380
Yellow
White
Blue
Red
White
Green

nearly to the red end of the spectrum. Professor Holmgren ('Proceedings of the Royal Society') states that the red seen by the violet-blind eye, in his case of one-sided violet-blindness, is not quite identical with the common spectral red of the normal eye, but rather a clearer red having a shade of carmine, about the same as the red towards the end of the subjective spectrum of the normal-eyed. This colour would obviously require a slight admixture of yellow to reduce it to the common spectral red of the normal-eyed. Moreover the extension of the yellow nearly to the red end of the spectrum explains how it is that a great many persons who are red-green colour-blind can see almost to the red end of the spectrum.

The strip of red at the violet end of the spectrum also requires explanation. The colours seen by the red-green blind eye in the case of one-sided red-green blindness are not yellow and violet, but yellow and indigo with only a faint shade of violet in it. Indeed, while Professor Holmgren speaks to the faint shade of violet ('Proceedings of the Royal Society'), Professor Hippel, who discovered this case and had more opportunities of examining it, states that the blue lines of indium and cæsium (which are indigo, not violet) appeared the same to both eyes (Gräf's *Archiv*, vol. xxvii.). Therefore some addition is necessary to produce the deeper violet tints of the spectrum; and this can only be obtained by supposing that the violet rays affect the red colour-sense as well as the blue one.

This hypothesis, that violet results from combining the blue and red colour-sensations, is part of Professor Preyer's theory (see Sect. 38 of his paper in Pflüger's *Archiv*), and seems to me to be supported by several other facts.

(1) As I have already mentioned, when an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than the sensibility to red and green. On the other hand, if the angular magnitude of a coloured object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow. In each case violet behaves like a compound of blue and red. As the image moves towards the circumference of the retina, the violet object passes through blue into white, the red fading

first; while, as the angular magnitude of a violet object is diminished, it becomes reddish (v. Kries, *Gesichtsempfindungen*, p. 93).

(2) Again, Nuel (*Annales de l'Oculiste*, 80, 82, cited by v. Kries, p. 154) describes how in a certain case of acquired colour-blindness "violet appears blue, red and green white." Similarly Schön states that in cases of atrophy of the optic nerve, when green, red, and yellow are no longer recognized, blue alone is correctly designated, and violet is distinguished as dark blue (*Lehre vom Gesichtsfelde*, p. 23, cited by von Kries, p. 155).

(3) I have already mentioned a case of yellow-blue blindness described by Stilling, in which blue and violet were, in a faint spectrum, designated as red, though in a brighter spectrum they seem to have appeared grey.

All these facts seem to me to point to violet being the result of affecting at once the blue and red colour-senses.

I am moreover disposed to think that, in addition to the two pairs of complementary colour-senses, there is a fifth colour-sense for white.

Inasmuch as

$$R + G = Y + B = \text{White},$$

we have the two linear equations between five colour-sensations which are required to satisfy the laws which Maxwell and Helmholtz established. Therefore the hypothesis of a fifth, white, colour-sense is admissible.

That the eye does perceive white separately from any other colour is rendered at least probable by considering some particular cases in which this seems to occur.

(1) When an object is viewed indirectly, so that the image falls upon a part of the retina at a sufficient distance from the yellow spot, it will appear white or grey, whatever its actual colour may be (von Kries, pp. 91–95).

(2) If the angular dimensions of a coloured object be diminished, it will ultimately appear white or grey (von Kries, pp. 87, 94).

The more probable explanation in both these cases seems to be that the other colour-senses are no longer affected by the object, and only the white colour-sense remains affected by it.

(3) Every colour when intensely lighted up appears white (von Kries, p. 81). A not improbable explanation of this seems to be that the other colour-senses are only capable of being affected by light to a limited extent as compared with the white colour-sense.

(4) In cases of atrophy of the optic nerve the perceptions of different colours are gradually lost, until at length every colour appears grey (von Kries, p 154).

(5) There are also cases of total colour-blindness from birth, when every thing appears of the same colour with only different degrees of light and darkness. When this affects both eyes completely, it is of course impossible to predicate with absolute certainty what colour is perceived. But Becker (Gräf's *Archiv*, vol. xxv.) describes a case where only one eye was so affected, the other having normal vision; and I have seen another case described in which one half of each eye was completely colour-blind, the other half being normal. In each of these cases the colour-blind vision was of white. This white vision must have been arrived at either through the other colour-senses having been lost, leaving a white colour-sense behind, or through their having been modified into white.

XXVI. *On winding Electromagnets. By* Professors W. E. Ayrton, *F.R.S., and* John Perry, *M.E.**

[Plates X. & XI.]

The following experiments were made to determine which mode of winding a given length of wire on an iron bar gave the strongest electromagnet for the same current. Four bars of iron, each 12 inches long, were cut from the same rod $\frac{3}{8}$ inch thick; and an exactly equal length of wire was wound on the four bars respectively, in the following way:—

1. Wire wound equally over the whole length (Pl. X. fig. 1).
2. Wire coned towards each end (fig. 2).
3. Wire wound equally over half the iron bar, leaving the other end bare (fig. 3).

* Read December 9, 1882.

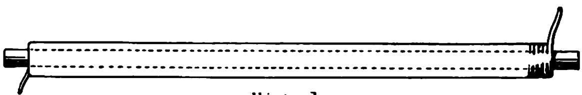


Fig. 1.

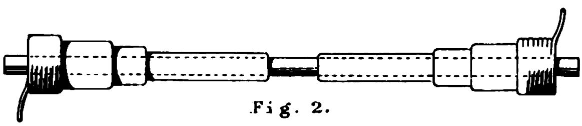


Fig. 2.

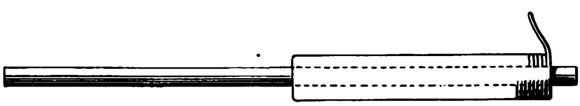


Fig. 3.

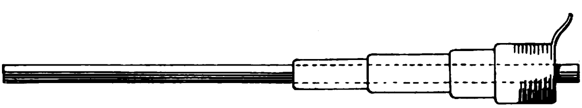


Fig. 4.

"WINDING ELECTROMAGNETS".

CURVES BETWEEN
TANGENT OF DEFLECTION OF NEEDLE AND
DISTANCE OF CORE FROM NEEDLE.

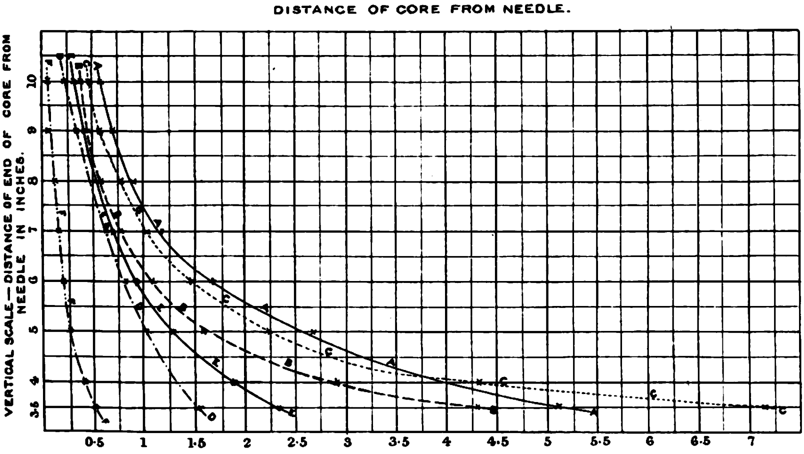
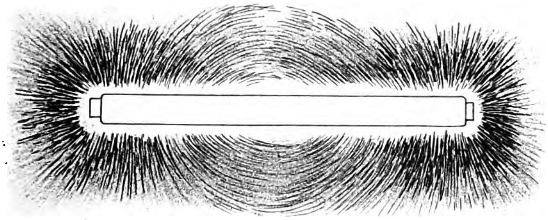


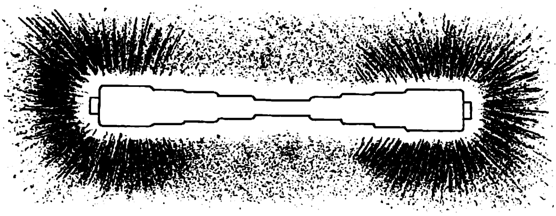
Fig. 5.

Mintern Bros. lith.

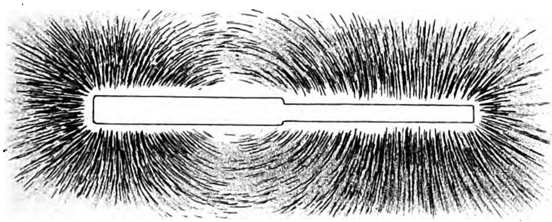
"WINDING ELECTROMAGNETS."
LINES OF FORCE AS SHOWN BY IRON FILINGS.



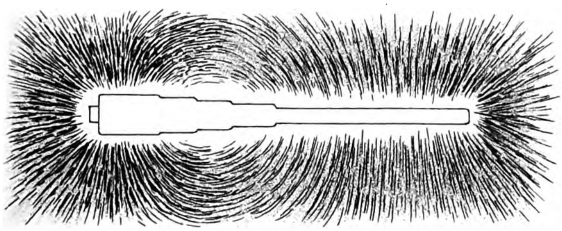
No. 6.
WOUND REGULARLY OVER WHOLE LENGTH.



No. 7.
WOUND CONED TOWARDS EACH END.



No. 8.
WOUND REGULARLY OVER HALF LENGTH.



No. 9.
WOUND CONED OVER HALF LENGTH.

Mintern Bros. lith.

4. Wire wound on one half but coned towards the end (fig. 4).

Electromagnet No. 1 was put so that its axis was at right angles to the axis of a small magnetic needle and passed through the point of suspension of the needle, which was suspended so as to move freely in a horizontal plane, and far enough away that the magnetic field due to the electromagnet No. 1, when magnetized by passing a current through it, was nearly constant over that portion of the field in which the little suspended needle moved when deflected. A constant current was now passed through the coil on No. 1, and the deflection of the little needle observed when No. 1 was placed at different distances from the centre of the test-needle, the axis of No. 1, however, always remaining in the same line. Under these circumstances it is well known that the strength of the field produced by No. 1 at the centre of the test-needle is approximately proportional to the tangent of its deflection. Experiments were now made in a similar way with electromagnet No. 2, and with each end of No. 3 and of No. 4, the same current as was employed with electromagnet No. 1 being used in all cases, and which was much below the saturating current.

The results obtained are given in the accompanying table, and are shown plotted in the accompanying curves (fig. 5), vertical distances representing the distance between the near end of the electromagnet and the centre of the test-needle, and horizontal distances the tangents of the deflection of the test-needle: A A A A is that for No. 1; B B B B for No. 2; C C C C for the covered end of No. 3; D D D D for the uncovered end of No. 3; E E E E for the covered end of No. 4; and F F F F for the uncovered end of No. 4.

Distance in inches between the near end of the bar and the centre of the test-needle.	No. 1.		No. 2.		No. 3.				No. 4.			
					Covered end.		Bare end.		Covered end.		Bare end.	
	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.
3½	79°	5·14	77°	4·33	82°	7·12	57°	1·54	67°	2·30	27°	0·57
4	77	4·33	71	2·9	77	4·33	53	1·33	62	1·88	21	0·38
5	69½	2·67	58	1·6	66	2·24	46	1·04	52	1·28	14	0·28
6	59	1·66	47	1·07	56	1·48	39	0·81	43	0·93	11	0·19
7	50	1·19	37	0·76	46	1·04	32	0·62	36	0·73	9	0·16
8	42	0·9	30	0·58	37½	0·79	27½	0·52	29	0·56	7	0·12
9	35	0·7	24	0·46	30	0·58	18	0·32	22	0·48	4	0·07
10	30	0·58	20	0·36	25	0·47	13	0·23	17	0·31	3	0·05

To ascertain the distribution of the lines of force, iron filings were sprinkled on paraffined paper, and the positions the filings took up fixed by the paraffin being softened by a heated piece of copper being passed over the paper at a short distance above it. These fields of force are shown in the diagrams 6, 7, 8, and 9 (Pl. XI.). From the curves in fig. 5 and from the iron-filing curves it is seen that the effect of coning the wire is to produce a strong field very near the pole, but that the force falls off very rapidly as the distance from the pole increases. With No. 2 magnet, for instance, the field between the poles is so weak that scarcely any definite arrangement of filings is traceable in the diagram 7 corresponding with it.

From the curves in fig. 5 it is seen that, at considerable distances from the end of the electromagnet, the uniformly coiled magnet No. 1 produces the most powerful field, while for points nearer the magnet, but still at a distance of about 3 inches from it, the covered end of No. 3 magnet, corresponding with the curve C C C, produces the strongest field, the next strongest being produced by the magnet No. 2 with the wire coned towards each end, since obviously the curve B B B cuts the curve A A A at a point corresponding with a distance of about 3 inches from the end of the magnet. For distances very close to the magnet, this method of experimenting cannot, of course, be employed to measure the resultant force accurately; and hence observations by this method at distances of less than 3½ inches from the end of the magnet to the centre of the oscillating needle were not made, and conclusions as to the resultant magnetic force very close to the poles must, of course, not be drawn from the curves in fig. 4.

Returning to the curves taken up by the iron filings, we see that No. 1 magnet gives an arrangement similar to that of an ordinary regularly magnetized bar-magnet. With No. 2 the lines around the poles are similar to those of No. 1, but the field between the poles is very weak. Magnets Nos. 3 and 4 give very similar figures, showing a very peculiar distribution of force. There is a great concentration of the lines at the pole corresponding to the half of the iron which is covered with wire; but the unwound end seems to form a

long weak pole, with its maximum force near the centre of the bar, *i. e.* at the inner end of the coil,—the differences between these two being, that with No. 4 magnet there is, comparatively, a greater concentration of force at the wound pole, and that the opposite pole is longer and extends a little way into the coil—the result of the coning of the wire. In these two cases the unwound end of the iron seems to act like an armature.

To ascertain the force which each magnet would exert on an armature, experiments were made and the following results obtained, the current flowing through the coil in each case being exactly the same, as well as the armature employed:—

Magnet.	Weight required to detach the armature from the covered end of the magnet.
No. 1	45 ounces.
2	57 "
3	57 "
4	77 "

These results confirm those previously obtained, that the field produced by the covered ends of the electromagnets numbers 2 or 3 at distances near the pole is much stronger than that produced by No. 1. But they show something else, viz. that for very small distances it is the covered end of No. 4 that produces the strongest field. In other words, returning to fig. 5, the curve *EE*, although much below the curves *AA*, *BB*, and *CC*, must rise rapidly and cut the others, just as the curve *CC* cuts the curve *AA*, at a point corresponding with a distance of about 4·2 inches from the end of the magnet, and just as, again, the curve *BB* cuts *AA* at a point corresponding with a distance of about 3·2 inches from the end of the magnet. The curves of iron-filings (fig. 9) indeed give indication of the great strength and concentration of field there is produced close to the iron by the wire coned at the end, as employed in the magnet No. 4.

With, then, a definite iron core, a definite length of wire to be coiled on it, and to be traversed with a definite current, the mode of coiling to produce the largest field depends entirely on the distance from the end of the electromagnet at which the field is to be produced. With the particular magnet we

have employed we see that, at distances from the end of the magnet very small compared with the length of the core, the wire should all be coiled up at the near end of the core, as in fig. 4; to create a field at a distance from the end of the magnet equal to about a third of the length of the magnet, it is better to coil the wire uniformly over one half of the core, as in fig. 2, than to cone it up at the near end as in 4; while for distances from the end of the magnet equal to, or greater than, about $\frac{4}{5}$ of the length of the core, the uniform mode of winding is the best.

We have to thank two of our students, Messrs. Sayers and Pink, for most cordial assistance rendered us in this investigation.

XXVII. *Experiments on the Viscosity of a Solution of Saponine.* *By* W. H. Stables *and* A. E. Wilson, *Yorkshire College, Leeds* *.

M. Plateau has shown (*Statique des Liquides*, t. 2, ch. vii.) that a body placed in a liquid and wetted on one side only, experiences in many cases a greater resistance to its motion than if it were completely immersed. Some controversy has arisen between Marangoni and himself as to the cause of this phenomenon, which M. Plateau explains by the assumption that the liquids in question possess a surface viscosity greater than that of the interior. The liquid in which the surface resistance is in most striking contrast to that of the interior is a solution of saponine in water.

Oberbeck (Wiedemann's *Annalen*, Bd. 11, S. 634) has repeated and extended Plateau's experiments, using an oscillating disk instead of a magnetic needle. He made no observations upon saponine solution. The object, therefore, of the following investigation is to study the movements of a disk when oscillating in or near the surface of a solution of this substance.

Oberbeck found that the resistance of a water-surface increased largely with exposure to the air; but he also proved that even with fresh distilled water the resistance is considerable. As he points out, we are therefore led to one of

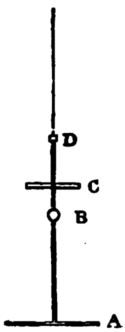
* Read April 14, 1883.

two conclusions—namely, that either water has a surface viscosity different from that of the interior, or else that a pure water surface cannot be obtained.

The apparatus used in our experiments was similar to that employed by Grotrian (Pogg. *Ann.* Bd. 157, S. 237).

It is fully described by him; and it is therefore unnecessary to figure here the connexions of the different parts. The diagrammatic representation in fig. I. may, however, conduce to clearness.

Fig. I.



It consists of a circular disk A of nickel-plated brass 76·25 millim. in diameter and 2 millim. thick. In the centre is screwed a brass rod, to which a concave mirror B and a small iron bar C (used to set the apparatus in motion by means of a magnet) are attached. A wire (119·8 centim. long), employed to put the apparatus in motion by torsion, is firmly gripped between two small plates at D.

The brass rod by which the plate was suspended could be unscrewed and replaced by others of different sizes.

The plate was suspended in the centre of a circular glass dish 15·5 centim. in diameter and 8 centim. deep. This was fitted with a wooden cover, through which a thermometer was introduced. The whole was surrounded by a case having glass sides, which served to ward off air-currents. The time of the oscillations was measured by a stop-watch indicating quarter-seconds.

Three rods were used in turn to suspend the plate. They were of the same length, but of different radii.

The radius of (1) was ·1337 centim.
„ (2) „ ·2360 „
„ (3) „ ·3362 „

The scale on which the beam of light was reflected was about 2·5 metres from the mirror; the maximum half-amplitude was about 200 millim.; so that the half-oscillation was about 2°·3.

The moment of inertia of the apparatus was determined by means of a brass ring of the same external diameter as the plate, and was found to be 753·09 (C.G.S.).

The plate was first carefully levelled, and distilled water poured into the vessel to within a few millimetres of the bottom of the plate. Water was then added with a pipette until the bottom of the plate and the surface of the water touched.

By a simple calculation the difference in height caused by the addition of a measured quantity of water was determined. An addition of 2 cubic centim. of water was found to cause a rise of ·1 millim. in the level.

The elevation was of course increased when the disk was in the surface; but the change is allowed for in the calculations.

The temperatures of the air and of the water were carefully noted at the commencement of each observation, and were found to remain nearly constant. To attain this end the experiments were conducted in a cellar, and special precautions were taken to prevent any marked variations in temperature. The variations therefore did not exceed 1° C.; and the small corrections thus rendered necessary were made by means of the table of the values of the coefficient of viscosity of water at different temperatures given by Grotrian (Pogg. *Ann.* Bd. 157, S. 242). All the observations were thus reduced to 16° as a standard temperature. The time of oscillation was ascertained by observing the time of 10 swings. This operation was repeated a number of times and the mean taken. The logarithmic decrement was calculated from the readings obtained from the graduated scale.

The plate was suspended by each of the three rods in turn immersed to a depth exceeding 1 centim., and the logarithmic decrement and mean time of oscillation determined for each. The following table shows the results obtained with the saponine solution and with water:—

TABLE I.

	Water.		Saponine.	
Rod.	Time of oscillation.	Log. dec.	Time of oscillation.	Log. dec.
	sec.		sec.	
1...........	5·19	·0477	5·24	·0785
2...........	5·20	·0479	5·26	·1424
3...........	5·22	·0483	5·26	·2045

The great difference between the surface-properties of saponine and those of water is here made very evident. The alterations in the dimensions of the rod which produced a slight effect only in the case of water increased the logarithmic decrement in the case of saponine two and a half times, a result which could only have been due to the increase of the section in contact with the surface.

If we assume that the disk oscillated under the influence of two forces, one of which (that of torsion) is proportional to the angular displacement from the position of rest, while the other, due to the viscosity of the liquid, is proportional to the velocity, the latter is measured by $M\lambda/T$, where M is the moment of inertia, λ the logarithmic decrement, and T the time of an oscillation. If, as in the case of a saponine solution, the surface resistance be so great that the friction between the surface layer and the interior may be neglected with regard to it, $M\lambda/T$ would be approximately of the form $a+br^2$, where r is the radius of the rod and a and b are constants.

The values of these, determined from the above equations by the method of least squares, are $a=8{\cdot}42$, $b=191{\cdot}6$.

Using these coefficients to calculate the value of $M\lambda/T$ from the observations on the saponine solution, we obtain the following results:—

TABLE II.

r.	$M\lambda/T$.	
	Observed.	Calculated.
0		8·42
·1337	11·27	11·85
·2360	20·37	19·07
·3362	29·36	30·07

The numbers are perhaps in as good agreement as could be expected, if we remember that the theory on which they are calculated is only approximate. If in the case of water we neglect b, the value of a is 6·92.

After the above preliminary observations a careful series of

experiments was made in which the plate was gradually immersed to a greater depth in water.

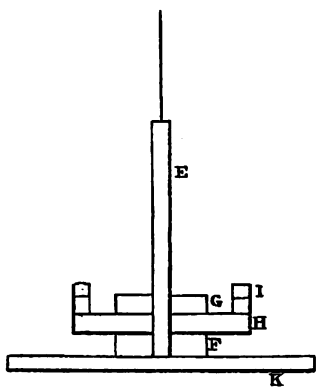
The following are the results obtained :—

Position of plate.	Time of oscillation.	Log. decrement.
Upper edge of plate ·7 mm. *above* level of water	sec. 5·06	·0258
·56 millim. above	5·10	·0259
·28 " "	5·10	·0260
·14 " "	5·11	·0262
Top of plate level with surface of water	5·10	·0266
·1 below	5·12	·0272
·2 "	5·11	·0272
·3 "	5·13	·0285
·4 "	5·17	·0298
·5 "	5·18	·0329
·7 "	5·20	·0366
·9 "	5·20	·0415
1·3 "	5·20	·0479
1·7 "	5·20	·0488

In the above results, the level of the liquid is corrected for the displacement of the liquid by the plate.

Similar experiments were next made on the saponine solution; but it was at once seen that the apparatus used in the previous experiment with the water was quite unsuitable on account of the great resistance offered by the surface. Certain modifications were therefore introduced. In this second form (fig. II.) a steel piano-wire was used, the length of which was 98·35 centim. and the diameter 1·025 millim. Suspended by this wire was a stout brass rod, E, terminating in a disk, F, upon which rested a similar movable disk, G; and between the two disks could be fitted a brass plate, H, carrying heavy brass rings, I. To the bottom of the lower disk an (unmagnetized) steel bar K was attached, by means of which the apparatus was set in oscillation by the use of a

Fig. II.



magnet. The apparatus used in the water experiment was attached to the centre of this bar by a strip of metal attached to D (fig. I.) and soldered to the bar. Great care was taken to render the junction perfectly firm, so that no torsion could possibly take place at this point. The moment of inertia was determined as before, and found to be 186653 (C.G.S.).

The first fact which was evident from the experiments was that, although the apparatus when suspended in water oscillated isochronously, it did not do so when suspended in the saponine solution. The following table gives the results of a number of experiments on the time of two long and two short swings respectively in that solution:—

Large amplitude.	Small amplitude.
10·90 sec.	10·28 sec.
10·70 „	10·28 „
10·75 „	10·35 „
10·52 „	10·35 „

With a thicker rod:—

10·42 sec.	9·85 sec.
10·52 „	9·73 „
10·33 „	9·90 „

Similar experiments in the case of water gave for two long oscillations, 10·25; for two short, 10·30; for three long oscillations, 15·5 and 15·75; and for three short, 15·6 and 15·75 seconds.

The amplitudes denoted long and short are not all of equal size; and as those called "large" are much larger than those actually used in the experiments, the correction which might otherwise have been necessary has been neglected.

The following are the results of the experiments on saponine solution:—

Position of plate. Upper surface	Time of oscillation.	Log. decrement.
·14 millim. above liquid...	9·51 sec.	·1960
level........................	9·50 „	·2520
·1 below surface	9·55 „	·0067
·2 „	9·60 „	·0045
·3 „	9·63 „	·0039
·4 „	9·58 „	·0034
·6 „	9·55 „	·0030
1·0 „	9·62 „	·0025
1·4 „	9·63 „	·0022
2·4 „	9·62 „	·0020
3·65 „	9·59 „	·0019

These results are shown on the accompanying curve (p. 241).

With regard to the first two observations, in which the plate was oscillating in the surface of the liquid, only two complete oscillations were obtainable for each determination; and as the logarithmic decrement was found to diminish considerably as the amplitude increased, a number of observations at different amplitudes were taken. These were plotted down in the form of a curve, showing the amplitudes and logarithmic decrement; and from these curves the logarithmic decrement for an arbitrary standard initial amplitude of 500 divisions was taken. The slope of these curves was so considerable that our observations can only be considered as giving an inferior limit to the resistance of the surface of the saponine solution. When the plate was once immersed below the surface, it was found that twenty or more oscillations were readily obtained, and that the magnitude of the original amplitude had little or no effect. The variations of temperature were small (the difference being only $0^\circ\cdot 7$ C.); and as their effect on the surface-viscosity is unknown, no correction was made for them. The error thus introduced would, however, as the regularity of the curve shows, be small.

These observations, then, enable us to compare the resistance offered to a disk when oscilating in, or just below, the surface of a saponine solution and of water.

Thus we get for the surface of saponine,

$$\frac{M\lambda}{T} = \frac{186653 \times \cdot 252}{9\cdot 5} = 4951;$$

and for the surface of water,

$$\frac{M\lambda}{T} = \frac{753 \times \cdot 0266}{5\cdot 1} = 3\cdot 927.$$

At ·1 millim. below the surface these numbers change to 131 and 4 respectively. At the surface, therefore, the ratio of the resistances is 1261; and at ·1 millim. below it is 33; while in the interior it is, as has been shown, $\frac{8\cdot 42}{6\cdot 93}$, or 1·2. Although, therefore, these numbers can only be taken as approximations to the truth, we think that they enable us to make an estimate of the magnitude of the resistance offered to a body

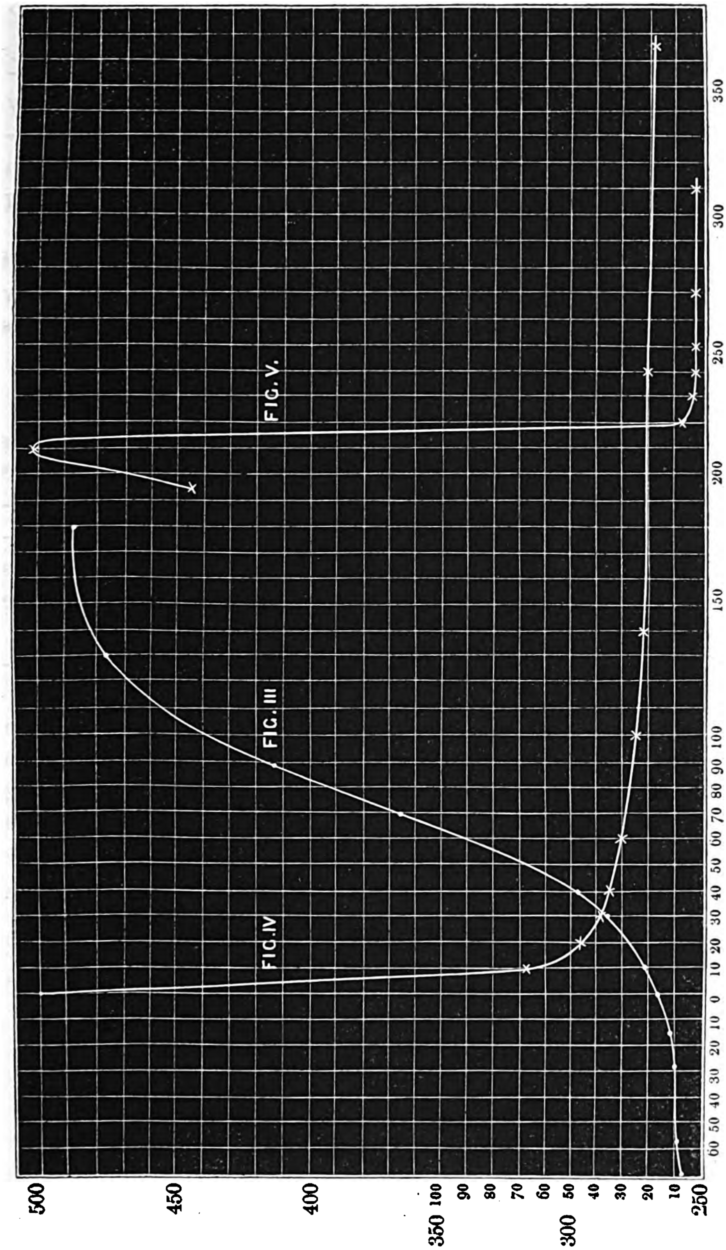


FIG. V.
FIG. III
FIG. IV
500
450
400
350
300
250
100
90
80
70
60
50
40
30
20
10
60 50 40 30 20 10 0 10 20 30 40 50 60 70 80 90 100
150
200
250
300
350

oscillating in the surface of saponine solution, for which no previous experiments afforded the required data.

They show that whereas the resistance offered to an oscillating disk, 2 millim. thick, in the surface of water is only about half what it is in the interior, at the surface of a 2-per-cent. saponine solution it is at least 600 times greater than in the interior, but that this ratio is reduced to 16 by immersing the upper surface of the disk to a depth of 0·1 millim.

Special experiments proved that the logarithmic decrement in air was so small that the resistance of the air might safely be neglected when the comparisons of the various resistances were made as above described.

Explanation of the Curves.

Fig. III. is the curve given by the logarithmic decrements obtained from the experiments on water. The abscissæ are expressed in terms of hundredths of a millimetre; they represent the distance of the upper edge of the plate from the surface, and are negative when it is above it. The ordinates represent the logarithmic decrement in terms of ·0001, the lowest horizontal line corresponding to the value of ·0250.

The gradual increase in the value of the logarithmic decrement as the plate is more deeply immersed is clearly shown.

Fig. IV. refers to the observations made on the saponine solution. In this case the values of the ordinates are to be taken from the small figures. The positions corresponding to the numbers obtained when the disk was in the surface cannot be shown on the scale of the diagram. Fig. V. therefore has been drawn on one tenth of the scale of fig. IV. To avoid confusion it has been displaced to a convenient distance along the line of abscissæ. The enormous increase of resistance as soon as the disk touches the surface is very strikingly shown; and it must be remembered that the increase for a very small oscillation would be very much greater.

The conclusion may be drawn from figs. III. and IV., that both in water and in the saponine solution the effect of the surface disappears when the edge of the disk is about a millimetre and a half below it.

We cannot conclude without expressing our sincere thanks to Prof. Rücker for his kind assistance in our experiments.

XXVIII. *On Curved Diffraction-gratings.* *By* R. T. Glazebrook, *M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator at the Cavendish Laboratory, Cambridge**.

Prof. Rowland has described the appearances presented when a beam of light, after passing through a slit, falls on a grating ruled on a cylindrical surface, and has given a very elegant construction for determining the position of the diffracted foci in the case in which the principal section of the grating is a circle and the source of light is placed at its centre of curvature. The mathematics of the subject have been dealt with still more recently by M. Mascart (*Journal de Physique*, January 1883) and Mr. W. Baily (Phil. Mag. March 1883). The object of the present paper is to carry the discussion somewhat further.

Prof. Rowland claims for his gratings that they enable him to form a pure spectrum without the use of lenses, and hence have an immense advantage over those hitherto employed. It must, however, be remembered that the formulæ obtained to give the position of the diffracted spectra are only true to a first approximation, that the spectra formed and the source of light are to one another in the relation of the conjugate geometrical foci of a lens or mirror. All the waves which arrive at any one point of the spectrum are not in exactly the same phase. Aberration effects are produced, and have to be considered just as in the ordinary theory of lenses or mirrors. Now, if a plane wave of light fall on a plane grating, and the effects be observed on a screen at an infinite distance behind the grating, the spectrum formed is perfectly pure; all the red light, after passing the grating, is definitely brought to a focus at one point; there is no aberration, so far at least as the grating is concerned. Of course the difficulty is to obtain the plane wave and the screen at an infinite distance. If the source of light be placed at the principal focus of a collimating lens, the emergent wave differs from a plane by quantities depending on the aberration of the lens; while if the diffracted beam is received on a second lens and a screen be placed in the focal plane of that lens, the screen would

* Read April 14, 1883.

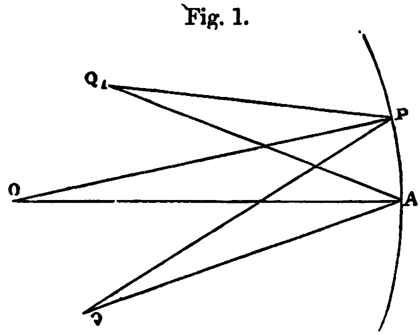
practically be at an infinite distance from the grating but for the aberration produced by the lens.

So far, then, as definition merely is concerned, we have to compare the aberration effects produced by these lenses with those caused by the curvature of the grating. Of course a reflexion grating used without lenses has an immense advantage for experiments on the violet or ultra-violet rays which are absorbed by glass.

In considering the aberration, then, we shall follow the method adopted by Lord Rayleigh in his paper on "Investigations in Optics, with special reference to the Spectroscope. Aberration of Lenses and Prisms" (Phil. Mag. January 1880).

Let QA, QP be two adjacent rays diverging from a point Q and falling on the concave side of a circle AP, centre O. Let $QAO = \phi$, $AOP = \omega$, $QA = u$, $OA = a$. Then

Fig. 1.



$$QAP = \phi + \frac{\pi}{2} - \frac{\omega}{2},$$

$$AP = 2a \sin \frac{\omega}{2}.$$

Hence

$$QP^2 = u^2 + 4a^2 \sin^2 \frac{\omega}{2} - 4au \sin \frac{\omega}{2} \sin \left(\frac{\omega}{2} - \phi\right); \quad . \quad . \quad (1)$$

and, expanding as far as ω^3, we find

$$QP = u + a\omega \sin \phi - \frac{a\omega^2}{2} \left(\cos \phi - \frac{a}{u} \cos^2\phi\right)$$
$$- \frac{a\omega^3 \sin \phi}{2} \left(\frac{1}{3} - \frac{a}{u} \cos \phi + \frac{a^2}{u^2} \cos^2 \phi\right) + \ldots \quad (2)$$

Again, let Q_1 be another point on the other side of the normal OA, and let $Q_1A = u'$, $Q_1AO = \psi$. Then

$$Q_1P = u' - a\omega \sin \psi - \frac{a\omega^2}{2}\left(\cos \psi - \frac{a}{u'} \cos^2 \psi\right)$$
$$+ \frac{a\omega^3 \sin \psi}{2} \left(\frac{1}{3} - \frac{a}{u'} \cos \psi + \frac{a^2}{u'^2} \cos^2 \psi\right). \quad . \quad . \quad (3)$$

Suppose now that A is a point on one line of the grating, and P a corresponding point on some other line. Then waves from Q diffracted at A and P respectively will reach Q_1 in the same phase if $QP + Q_1P = QA + Q_1A \pm n\lambda$, λ being the wave-length. That is, if

$$\begin{aligned} &a\omega(\sin\phi - \sin\psi) \\ &\quad - \frac{a\omega^2}{2}\left\{\cos\phi + \cos\psi - a\left(\frac{\cos^2\phi}{u} + \frac{\cos^2\psi}{u'}\right)\right\} \\ &\quad - \frac{a\omega^3}{2}\left\{\sin\phi\left(\frac{1}{3} - \frac{a}{u}\cos\phi + \frac{a^2}{u^2}\cos^2\phi\right)\right. \\ &\quad \left. - \sin\psi\left(\frac{2}{3} - \frac{a}{u'}\cos\psi + \frac{a^2}{u'^2}\cos^2\psi\right)\right\} = \pm n\lambda. \quad . \quad (4) \end{aligned}$$

This is equivalent to Mr. Baily's formula carried to the next degree of approximation; and his results are obtained by neglecting the term in ω^3 and taking ϕ and ψ to satisfy the equation

$$\sin\phi - \sin\psi = \pm\frac{n\lambda}{a\omega}, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and then u and u' to satisfy

$$\cos\phi + \cos\psi - a\left(\frac{\cos^2\phi}{u} + \frac{\cos^2\psi}{u'}\right) = 0. \quad . \quad . \quad (6)$$

To consider the aberration we have two cases before us. Let us suppose (1) that equation (5) holds, and determine the value u'_1, say of u', considering the terms in ω^3 in equation (4). This will give us what we may call the longitudinal aberration.

In the second case we shall suppose equation (6) to hold, and determine the value for ψ which satisfies (4) to the same approximation. This will give us the lateral aberration.

In the general case equation (4), as it stands, really determines the locus of the image of Q formed by diffraction at the two lines A and P; and this locus is clearly an hyperbola, with A and P as foci. Waves diffracted at A and P respectively will arrive in the same phase at any point of this hyperbola. For every point such as P on the grating an hyperbola possessing similar properties can be drawn. If all these hyperbolas meet in a point, then that point is really a focus for waves diverging from Q; they all are in the same phase when they meet there. This is the case if the grating be plane and Q and Q_1 infinitely distant. If, however, the hyperbolas do not all meet in a point, there is really no focus in its strict sense, only a geometrical focus. If we neglect ω^3 and higher

terms, then the point given by (5) and (6) is to this approximation common to all the hyperbolas: it is the geometrical focus.

To discuss, then, the aberration in this case. Let $u'_1 = u' + \delta u'$, where u' satisfies (6), and suppose we neglect $\overline{\delta u'}|^2$, $a\omega\delta u'$, and such terms. Then

$$\cos\phi + \cos\psi - a\left\{\frac{\cos^2\phi}{u} + \frac{\cos^2\psi}{u'}\left(1 - \frac{\delta u'}{u'}\right)\right\}$$
$$+ a\omega\left\{\pm\frac{n\lambda}{3a\omega} - \frac{a\sin\phi\cos\phi}{u}\left(1 - \frac{a}{u}\cos\phi\right)\right.$$
$$\left. + \frac{a\sin\psi\cos\psi}{u'}\left(1 - \frac{a}{u'}\cos\psi\right)\right\} = 0.$$

Thus

$$\delta u' = -\frac{u'^2\omega}{a\cos^2\psi}\left\{\pm\frac{n\lambda}{3a\omega} - \frac{a}{u}\sin\phi\cos\phi\left(1 - \frac{a}{u}\cos\phi\right)\right.$$
$$\left. + \frac{a}{u'}\sin\psi\cos\psi\left(1 - \frac{a}{u'}\cos\phi\right)\right\}. \quad . \quad . \quad (7)$$

Equation (7) determines the aberration in the general case. To determine the effect of this in practice, let us suppose that we are considering the spectrum of the first order, so that the retardation of the light coming from two consecutive lines is just one wave-length; and hence, if P be on the kth line from A, and σ the distance between two lines, then the arc AP $= k\sigma = a\omega$, and $n = k$.

Let us suppose, further, that the origin of light is at the centre of curvature of the grating, so that $u = a$, $\phi = 0$, and hence, taking the $-$ve sign in (6),

$$\sin\psi = \frac{\lambda}{\sigma},\ \cos\psi = \sqrt{\left(1 - \frac{\lambda^2}{\sigma^2}\right)},\ u' = a\cos\psi = a\sqrt{\left(1 - \frac{\lambda^2}{\sigma^2}\right)}.$$

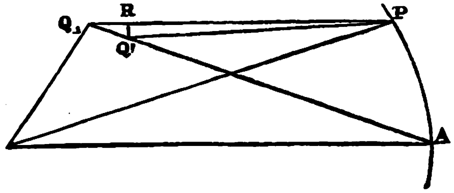
Thus

$$\delta u' = +a\omega\frac{k\lambda}{3a\omega} = +\tfrac{1}{3}k\lambda. \quad . \quad . \quad . \quad (8)$$

Let Q_1 (fig. 2) be the point on the line given by $\sin\psi = \frac{\lambda}{\sigma}$, which is determined by $u' = a\cos\psi$, Q' being the point on that line at which light arrives in exactly the same phase from A and P. Then $Q_1Q' = \frac{1}{3}k\lambda$. And the angle PQ_1A differs by only a small quantity from ω; while, since $Q'Q_1$ is small compared with PQ', the angle Q_1PQ' is small compared with ω.

Hence, if $Q'R$ be drawn at right angles to PQ_1, the light from P arrives at R in the same phase as at Q', and the difference

Fig. 2.



in phase at Q_1 between the waves coming from A and P is $Q_1Q' - Q_1R = Q_1Q'(1 - \cos\omega)$; and to the same approximation this is equal to $\frac{1}{6}k\lambda\omega^2$. So that if we consider as well the light coming from a point k' lines below A, the extreme difference of phase in the various waves which reach the point Q_1 is $\frac{1}{6}(k+k')\lambda\omega^2$; $k+k'$ will be the total number of lines in the grating.

Thus in one of Prof. Rowland's gratings we have

$$a = 213 \text{ centim.},$$

$$\omega = \frac{37}{2130} \text{ about},$$

$$k+k' = 14250;$$

and hence the difference in phase is about $7\lambda/10$. Hence the aperture of the grating is too large to give the best definition: for that purpose the difference of phase in the various secondary waves arriving at the point in question should not be greater than $\lambda/4$.

We may conveniently express this difference of phase in terms of the number of lines, the radius of the grating, and the distance between the lines. Let σ be the distance between the lines; then

$$\omega = \frac{(k+k')}{2a}\sigma,$$

and the difference of phase is

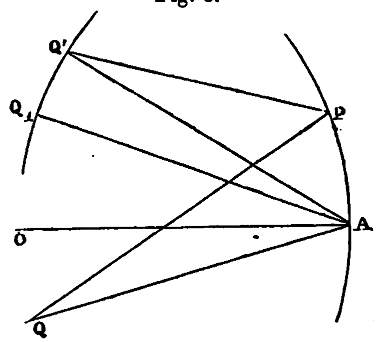
$$\frac{1}{24} \times (k+k')^3 \frac{\sigma^2}{a^2} \lambda.$$

For good definition this difference of phase must not be greater than $\lambda/4$. Since in the case above the difference of phase is $7\lambda/10$, we must reduce the number of lines, keeping the distance between them the same, in the ratio of $\sqrt[3]{10}$ to $\sqrt[3]{28}$, or

rather more than 2 to 3. Hence by covering up rather less than one third of the grating we should expect to produce better definition.

In another grating of Rowland's, $\sigma=\frac{1}{11400}$ centim., $a=520$ centim., $k+k'=160{,}000$; and in this case the difference of phase comes out to be about $4{\cdot}8\times\lambda$. The grating is much too wide; it will require reducing in the ratio of $1:\sqrt[3]{19{\cdot}2}$, or about 3 : 8.

Fig. 3.



To consider now the lateral aberration, using the same notation, describe a circle (fig. 3) through Q_1 with A as centre. Light from A arrives in the same phase at all points on this circle. Let Q' be the point on the circle at which the light arriving from P is in the same phase as that from A, and let $\psi+\delta\psi$ be the angle OAQ', and let ψ, ϕ, u, and u' satisfy (5) and (6). Our fundamental equation (4) becomes, neglecting terms like $\omega^2\delta\psi$,

$$\begin{aligned}a\omega\Big[&\sin\phi-\sin\psi-\delta\psi\cos\psi\\&-\frac{\omega}{2}\Big\{\cos\phi+\cos\psi-a\left(\frac{\cos^2\phi}{u}+\frac{\cos^2\psi}{u'}\right)\\&-\sin\psi\,\delta\psi\left(1-\frac{2a\cos\psi}{u'}\right)\Big\}\\&-\frac{\omega^2}{2}\Big\{\sin\phi\left(\tfrac{1}{3}-\frac{a}{u}\cos\phi+\frac{a^2}{u^2}\cos^2\phi\right)\\&-\sin\psi\left(\tfrac{1}{3}-\frac{a}{u'}\cos\psi+\frac{a^2}{u'^2}\cos^2\psi\right)\Big\}\Big]=\pm n\lambda. \quad (9)\end{aligned}$$

Hence

$$\begin{aligned}\delta\psi&\left\{\cos\psi-\frac{\omega}{2}\sin\psi\left(1-\frac{2a\cos\psi}{u'}\right)\right\}\\&+\frac{\omega^2}{2}\Big[\sin\phi\left\{\tfrac{1}{3}-\frac{a}{u}\cos\phi\left(1-\frac{a}{u}\cos\phi\right)\right\}\\&-\sin\psi\left\{\tfrac{1}{3}-\frac{a}{u'}\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\}\Big]=0\,;\end{aligned}$$

and, to the approximation adopted in considering the longitudinal effect,

$$\delta\psi = -\frac{\omega^2}{2}\sec\psi\left\{\pm\tfrac{1}{3}\frac{n\lambda}{a\omega} - \frac{a}{u}\sin\phi\cos\phi\left(1-\frac{a\cos\phi}{u}\right)\right.$$
$$\left. + \frac{a}{u'}\sin\psi\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\}. \quad . \quad . \quad (10)$$

If, as before, Q coincide with O, then $u=0$, $\phi=0$, $u'=a\cos\psi$; and taking the negative sign, so that $\sin\psi = \frac{n\lambda}{a\omega}$,

$$\delta\psi = \frac{\omega\sec\psi\times n\lambda}{6a}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

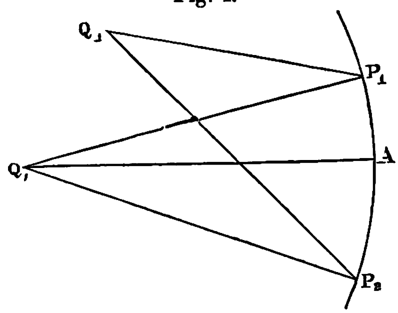
and $$Q_1Q' = u'\delta\psi = \frac{n\lambda\,\omega}{6}.$$

Thus, carrying our approximation as far as terms in ω^3 in equation (4), we find that the position of the image formed, considering only two of the lines as producing diffraction effects, is not at Q_1 but Q', where $Q_1Q' = \frac{n\lambda\omega}{6}$. Hence, if we consider the whole grating, using the same notation as before, the breadth in a direction normal to AQ_1 of the image formed will be comparable with $\frac{(k+k')\lambda\omega}{6}$, ω being the whole semi-aperture. Expressing this in terms of the radius of the grating a and the distance between the lines σ, we find the value $\frac{1}{6}(k+k')^2\frac{\lambda\sigma}{a}$. Thus the breadth of the image will depend on the square of the number of lines. In the grating first considered this quantity, $\frac{1}{6}(k+k')\lambda\omega$, is about $\frac{1}{500}$ of a centimetre for yellow light, while the distance between the D lines is about $\frac{1}{50}$ centim., or ten times as much; while in the second grating this lateral aberration is $\frac{1}{50}$ centim., the distance between the D lines being about seven times as great. If the size of this last grating be reduced to $\frac{3}{8}$ of what it actually is, the extreme lateral aberration will be reduced to $\frac{9}{64}$ or about $\frac{1}{7}$ of its actual value, thus becoming about $\frac{1}{350}$ of a centimetre, and the extreme difference of phase in the light of a given wavelength λ reaching any point of the diffracted spectrum will never exceed $\lambda/4$, the dispersion will remain unaltered, the definition and the brightness of the spectrum will both be increased.

It is clear that in both cases the outer portion of the grating

not merely impairs the definition, but actually renders it less bright than before. For consider two points P_1, P_2 (fig. 4) equidistant from A, such that the difference in phase in the waves coming from P_1 and P_2 to Q_1 is $\frac{\lambda}{2}$ (since the difference of phase for the extreme rays is in both cases greater than $\frac{\lambda}{2}$, these points can be found). Then the light reaching Q_1 from above P_1 is opposite in phase to some of that which reaches Q_1 from between P_2 and A, and tends to neutralize the effect of this; while similar results hold for light coming respectively from below P_2 and between A and P_1. Thus a large aperture does not necessarily mean that there is a large quantity of light at the focus. Exactly the same may happen in the case of a lens. Lord Rayleigh has shown that if α be the angular semi-aperture of the lens as viewed from the focus, and the curvatures of the lens be adjusted to reduce the longitudinal aberration to a minimum, α^4 should not exceed λ/f. A similar course of reasoning shows us that if α^4 is greater than $2\lambda/f$, the light from the outer annulus of the lens will be opposite in phase to that from the central portions.

Fig. 4.



To compare, finally, the definition of the curved grating with that produced by a plane grating, and two lenses of equal focal length used as a collimator, and the object-glass of a telescope respectively, we can show (Parkinson, 'Optics,' § 130), that if α is the semi angular aperture of either of these lenses seen from its principal focus, f its focal length, and the curvatures are adjusted to make the aberration of each lens a minimum, then the aberration is, for light of refractive index 1·5, $\frac{15}{7}f\alpha^2$; but, as quoted above, Lord Rayleigh has shown that the aberration should not be greater than λ/α^4. Hence α^4 must not be greater than $\frac{7\lambda}{15f}$.

In the case of the first of Prof. Rowland's gratings discussed above, the slit and eyepiece are at a distance of about 200 centim. from the grating. Let us suppose we are using two lenses of 200 centim. focal length, and inquire what their aperture may be to allow the condition above given to be satisfied. If y be the radius of the lens, we have

$$y^4 \text{ not } > \text{ than } \frac{7 \times 8 \times 10^6 \times 6}{15 \times 10^5}.$$

Thus y must not be greater than 3·8 centim. A lens of this aperture would just about admit the light from the whole of the actual grating 5 centim. × 7 centim. in area if it were plane; whereas, without the lens, to obtain the best definition we are restricted to the use of about two thirds of the grating.

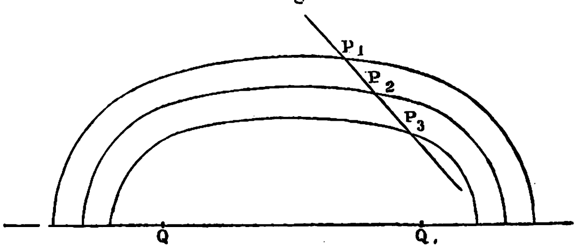
In the case of the other grating, we may, without increasing the size of the apparatus, use a lens of 500 centim. focal length; for good definition its aperture should not be more than

$$\sqrt[4]{\frac{7 \times 125 \times 10^6 \times 6}{15 \times 10^5}},$$

or about 7·6 centim. A lens of this aperture would enable us to use with the best advantage the whole of the grating if it were plane; whereas in the concave grating, for good definition we should only use about three eighths of the whole. It would seem, then, that in cases in which there is no objection to the use of glass (because of its absorbing qualities), a large grating may be used to greater advantage if it be ruled on a flat surface and properly chosen lenses be employed with it, than if the grating be curved.

It may be instructive to consider the subject briefly in another manner. Let Q, Q_1 (fig. 5) be any two points, and

Fig. 5.



with Q and Q_1 as foci describe a series of confocal ellipses; let the major axes of these ellipses increase in arithmetical progres-

sion, and let the common difference be λ. Consider a spherical wave diverging from Q and reflected at any point of any one of these ellipses; all the reflected light will reach Q_1 in the same phase. Take any surface P_1, P_2, &c. cutting the ellipses in P_1, P_2 &c., and suppose it capable of reflecting light at these points and incapable of so doing elsewhere. All the light from Q which falls on this surface at these points will be reflected to Q_1, and the various waves will reach Q_1 in the same phase. If now Q be the section of a slit normal to the paper, $P_1 P_2$ &c. that of a polished cylindrical surface whose generators are normal to the paper, and lines be ruled on this surface to block out the spaces $P_1 P_2$, $P_2 P_3$, &c., the lines also being normal to the paper, we shall obtain a diffraction-grating which will give an image of Q without aberration at Q_1.

We can thus determine the law according to which lines must be ruled on any cylindrical surface to give an aplanatic diffraction-image of a slit; for we require only to write down the equations to the ellipses and the surface and determine the points of intersection. We will solve the simple case when the curve $P_1 P_2$ &c. is a straight line parallel to $Q Q_1$. Take $Q Q_1$ as axis of x; let a and b be the semi-axes of one of the ellipses, suppose that which touches the line $P_1 P_2 \ldots$; then the semi major axes of the other ellipses are

$$a+\frac{\lambda}{2}, \quad a+\delta \ldots a+\frac{n\lambda}{2}, \text{ \&c.};$$

while to find b_n, the semi minor axis of the $(n+1)$th ellipse, we have

$$\left(a+\frac{n\lambda}{2}\right)^2-b_n^2=a^2-b^2 . \text{A} b_n^2=b^2+na\lambda+\frac{n^2\lambda^2}{4}. \quad . \quad (12)$$

Let $Q Q_1=2c$, then we have

$$a^2=b^2+c^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and the equation to the $(n+1)$th ellipse is

$$\frac{x^2}{a^2+na\lambda+\frac{n^2\lambda^2}{4}}+\frac{y^2}{b^2+na\lambda+\frac{n^2\lambda^2}{4}}=1. \quad . \quad . \quad (14)$$

Let x_n be the abscissa of the point in which this is cut by the line $y=b$, then

$$x_n^2=\frac{\left(a^2+na\lambda+\frac{n^2\lambda^2}{4}\right)\left(na\lambda+\frac{n^2\lambda^2}{4}\right)}{b^2+na\lambda+\frac{n^2\lambda^2}{4}}. \quad . \quad . \quad (15)$$

Substituting for a and giving n the values 0, 1, 2, 3, &c. in order, we can obtain values for x_0, x_1, x_2 &c., and determine thus the position of the lines. A plane grating ruled in this manner would form at Q_1 without aberration an image of Q for light of the given wave-length λ. Of course it would be open to the objection which holds against all such aplanatic arrangements, viz. that they are only good for light of one definite wave-length. If the grating were used for light of a different refrangibility, the image formed would suffer from aberration.

XXIX. *A new Photometer.*

By Sir JOHN CONROY, *Bart.*, *M.A.**

HAVING recently made a considerable number of photometric observations, and learnt by experience the difficulty which attends all such determinations, I venture to bring before the Society the description of a new form of photometer which appears to possess certain advantages over those in use. All such instruments, with the exception of the wedge-photometer, are essentially arrangements for comparing the illuminating-power of two lights, and therefore do not give absolute measures; the one I propose describing is no exception to this general rule.

I had intended to use, in some experiments on the amount of light reflected by metallic surfaces, the ordinary Bunsen's disk; but I found that, owing to the small size of the beam of reflected light, it was impossible to make any satisfactory measurements with the disks in common use, and after trying various photometric arrangements I finally adopted a modification of Ritchie's photometer.

The various forms of shadow-photometers work well; but as the accuracy of the determination depends on the edge of the two shadows coinciding and yet not overlapping, it is necessary to have some arrangement for altering the distance between the screen and the shadow-producer, which adds to the complexity of the apparatus, except indeed when, as in Mr. Harcourt's photometer for gas-work, the variation in the relative intensities of the two lights is caused by the size of one of the flames being altered, and not, as in those arrange-

* Read April 28, 1883.

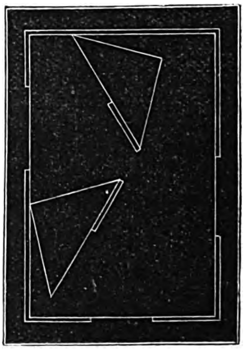
ments heretofore in use, by altering the distance of the flame from the screen whilst the size is kept constant.

Ritchie's photometer, as is well known, consists of two pieces of white paper fastened to the adjacent sides of a triangular block of wood, each being illuminated by one only of the lights which are to be compared. Finding it impossible so to arrange the apparatus that the illuminated surfaces should be actually in contact, the bend in the paper along the edge of the block separating the two illuminated areas, and therefore interfering with the accuracy of the determination, I placed one of the pieces of paper slightly in front of the other, and overlapping it to a small extent, so that, whilst both were visible to the observer, each was illuminated by one only of the sources of light; when equally illuminated, the edge of the front paper vanished.

It was originally intended that the light should be incident upon the surfaces of the paper at an angle of 45°; but it was found that when the light regularly reflected by the paper reached the observer (*i. e.* when the line of sight and the direction of the incident light formed equal angles with the normal to the paper) it was not possible to make satisfactory measurements.

After various positions had been tried, it was found that the best results were obtained when the light was incident upon the paper at an angle of about 30° and the line of sight formed an angle of 60° with the normal.

Two triangular blocks of wood, 4 centim. high, were screwed to a rectangular board about 15 centim. by 10 centim., in the position shown in the figure, and pieces of white paper, 3 centim. by 3 centim. (filter-paper was tried; but ordinary white writing-paper not too highly glazed seemed most suitable), held against the hypotenuse of each of the triangular prisms by india-rubber bands.



$\frac{1}{4}$ actual size.

It is of course essential that the light should be incident upon both papers at equal angles, and that the papers should be so placed that no light can

be reflected from one to the other. It is desirable that both papers should be cut from the same sheet, and that the surfaces on which the light is incident should originally have formed one side of that sheet.

A rectangular board, similar to that to which the prisms were fixed, was fastened to the top of the prisms by two screws; and to the edges of this board four strips of card, in three of which square apertures had been cut, were fixed, and the whole arrangement painted both externally and internally a dead black.

In order to adjust the papers, or replace them by new ones, it is merely necessary to withdraw the two screws in the top board and lift it off, together with the sides of the box.

The edge of the front paper coinciding with the middle line of the box, the photometer could be used with either side uppermost; and in order to be certain that the illumination of both papers was entirely due to light incident directly upon them, measurements of the relative intensity of two similar paraffin-lamps were made with the photometer in both positions; and it was found that the readings were identical.

The photometer was compared with a Bunsen's disk by placing it at the end of a horizontal board furnished with a scale, and along which a paraffin-lamp was arranged to slide. A Bunsen's disk, in an ordinary form of support with two inclined looking-glasses, could be screwed to the end of the board, to which three stops were so fixed that, when the disk was removed and the new photometer placed against the stops, the middle line of the box was in the same vertical plane as the disk had been.

A paraffin-lamp was placed on either side of the photometer, the position of one remaining constant, whilst that of the other was altered until the illumination was equal, and the distance of the latter read off, in centimetres, on the scale.

The table gives the results of eight observations made with both photometers, the differences of each observation from the mean, and also the squares of these differences.

Bunsen's Disk.

centim.		Differences from the mean.	Squares of the differences.
85·7	....	+ ·6	·36
84·6	....	− ·5	·25
84·9	....	− ·2	·04
85·4	....	+ ·3	·09
86·2	....	+1·1	1·21
84·8	....	− ·3	·09
84·3	....	− ·8	·64
85·2	....	+ ·1	·01
Mean . . 85·1		Sum . .	2·69

New Photometer.

85·0	....	−·4	·16
85·7	....	+·3	·09
85·0	....	−·4	·16
85·3	....	−·1	·01
85·2	....	−·2	·04
85·4	....	−·0	·0
85·8	....	+·4	·16
85·7	....	+·3	·09
Mean . . 85·4		Sum . .	0·71

The probable error of the mean result and the probable error of a single observation were found by the ordinary formulæ, $0{\cdot}6745\sqrt{\frac{\text{sum of the squares of the differences}}{n(n-1)}}$ and $\sqrt{n}\times$ the probable error of the mean result, n being the number of observations.

	Bunsen's disk. centim.	New photometer. centim.
Probable error of mean result	$\pm 0{\cdot}148$	$\pm 0{\cdot}076$
„ „ single observation	$\pm 0{\cdot}418$	$\pm 0{\cdot}215$

The new photometer therefore appears to be twice as accurate as the Bunsen's disk : it is only fair to add that, had the measurements been made by an observer accustomed to work with the disk, the result might have been different.

XXX. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VII. *By* C. R. Alder Wright, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and* C. Thompson, *Demonstrator of Chemistry, in St. Mary's Hospital Medical School**.

On the Electromotive Force of Clark's Mercurous-Sulphate Cell; and on the Work done during Electrolysis.

On the E.M.F. of Clark's Cell.

133. In the course of the series of experiments partly described in Parts V. and VI. a large number of observations have been made with various cells after Clark's construction (Proc. Roy. Soc. xx. p. 444), in all cases compared with one another and with other cells by means of the quadrant-electrometer only, so that they never generated any current other than the minute leakage current through the not mathematically absolutely insulating materials between their poles.

In some instances the mercurous sulphate was purchased (from Messrs. Hopkin and Williams), and was well washed before use by numerous boilings with distilled water and decantations. In other cases the mercurous sulphate was prepared by heating twice-distilled mercury (previously purified by nitric acid) with pure sulphuric acid at as low a temperature as possible consistent with any action taking place, and thoroughly washing the resulting sulphate by repeatedly boiling with distilled water and decantation. The action was never allowed to go on until more than a fraction of the mercury used was converted into sulphate, in order to reduce the amount of mercuric sulphate formed to a minimum.

The cells were made out of pieces of ordinary combustion-tubing (selected on account of the absence of lead in the glass) drawn out before the blowpipe into the U-shape represented on about two thirds scale in the cut (fig. 1). The glass being

* Read May 12, 1883.

perfectly dry and hot, pure recently-boiled still hot mercury was poured into them so as to form a layer about half an inch (10 to 15 millimetres) deep, *a*; on the top of this was then poured a boiling paste of thoroughly well-washed mercurous-sulphate and zinc-sulphate solution, containing so much of the latter salt as to be slightly supersaturated when cold, so as to crystallize on standing. It was found convenient to make the paste not too thick, and to let the solid matter subside in the cell, the supernatant comparatively clear fluid being sucked out by a clean pipette, so as finally to leave on the top of the mercury a layer of particles of mercurous sulphate wetted with zinc-sulphate solution some 15 to 20 millimetres deep, *b*. Pieces of zinc rod (cast in glass tubes from pure metal fused in a porcelain crucible), well brightened by a file that had never touched any other metal, were then placed in the cells so as to dip into the paste some 4 or 5 millimetres, and project out of it about twice as much, *c*. The zincs were kept from falling by pieces of cork, *d*, cut as represented in fig. 2, and previously immersed in hot paraffin-wax so as to expel air and moisture; to the ends projecting from the paste were previously soldered copper wires, *e*. Melted paraffin-wax was then poured into the cell so that all air was expelled, rising through the perforations in the edges of the cork disks, and so that the upper two thirds of the zinc and the soldering were completely covered, *f*. Finally, a piece of platinum wire, *g*, or a strip of foil was passed down the narrow limb of the cell so as to make contact with the mercury: it was found convenient to amalgamate the tip of the platinum by moistening it and immersing it in freshly made sodium amalgam, all sodium being removed from the adherent film of mercury by subsequent immersion in water for some hours. The cells thus prepared, being wanted for use only and not being required to be externally well finished, were not mounted in the neat

Fig. 1.

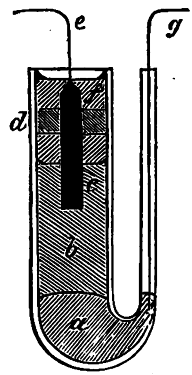
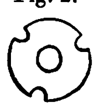


Fig. 2.



brass cases with ebonite tops and binding-screws usually employed, but were simply fixed in a beaker, or any other convenient holder, by pouring in melted paraffin-wax around them. When used in connexion with the electrometer, the copper wire soldered to the zinc and a similar wire soldered to the strip of platinum (and secured by a turn round the upper end of the narrow limb and a drop of sealing-wax) were bent over so as to dip into mercury-cups, a number of which were arranged in the arc of a circle round two others, like those figured in Part V.; so that any consecutive pair of cups could at will be connected with the electrometer by the double switch.

Either through a natural repulsion between bright zinc and the mercurous-sulphate paste, or through the formation of a faint film of grease &c. on the zinc from the file used to brighten it, it sometimes happened that the cell when finished would not work, contact not existing between the zinc and paste. It was found that this never occurred when the brightened zincs were washed successively with ether, alcohol, and saturated zinc-sulphate solution just before immersion in the paste.

134. On comparing together a moderately large number of cells (upwards of fifty) thus prepared with different specimens of mercurous sulphate, readings being taken two or three times a week for some three months, the following results were obtained:—A slight rise in E.M.F. was often observed during the first few days after construction; but at the end of a week at most the values *became constant, and remained so (the temperature being constant) for long periods of time.* The maximum variations observed between the average results of the series of observations for any two given cells were slightly less than that found to exist by Clark (whose highest and lowest values are respectively 1·4651 and 1·4517 volt, giving a difference of ·0134 volt, or upwards of 0·9 per cent.). Taking the average of the whole set as 100·00, the maximum variation between two single cells did not exceed ·010 volt, or 0·7 per cent., each cell possessing a value lying between 99·65 and 100·35. Even amongst cells set up at the same time from absolutely the same materials, extreme differences of as much as 0·005 volt = 0·35 per cent. were sometimes observed,

although usually the difference did not exceed ·002 or ·003 volt and was frequently almost inappreciable.

Much greater differences, however, were found to exist when the zinc-sulphate solution was not completely saturated with that salt, the variation produced being of this kind, that *the weaker the solution the higher the E.M.F. of the cell,* the difference being approximately proportionate to the amount of dilution, and amounting to upwards of 2·0 per cent. of the value when considerably dilute zinc-sulphate solution was used. The details of these observations will be discussed in a future paper, along with those of similar experiments made with other cells. It may, however, be here noticed that, so long as a cell containing unsaturated zinc-sulphate solution was protected against concentration by evaporation, and was only used in connexion with a quadrant-electrometer, its indications remained perfectly constant for many months (the temperature being the same), precisely as was found with cells set up with saturated zinc-sulphate solution.

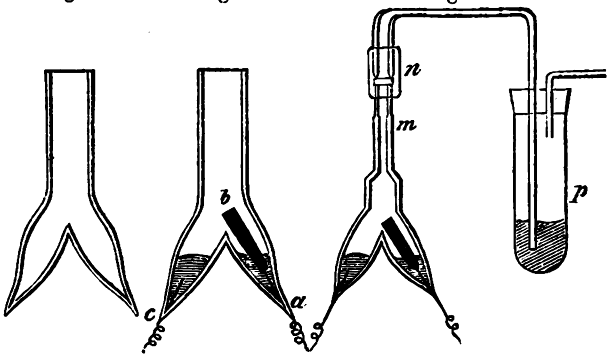
Effect of Dissolved Air on the E.M.F. of Clark's Cell.

135. Two series of experiments were made with the object of finding out how far the boiling of the mercurous-sulphate paste (as recommended by Clark) in order to remove dissolved air is essential. In one series a number of cells were set up, using fully aerated zinc-sulphate solution and unboiled mercury (exposed to the air under a glass shade for several days since preparation and distillation respectively); in the other the paste was boiled in a Sprengel vacuum produced in the cell itself for some time, the cell being then hermetically sealed, so as to reduce the amount of residual air to a minimum. In each case the average E.M.F. of the combination *was sensibly identical with that of an average ordinary Clark cell* prepared as above described and containing zinc-sulphate solution of the same strength as that contained in the combination.

In order to prepare these hermetically-sealed cells a rather troublesome process was employed. First a piece of glass tubing, about 10 or 12 millim. in bore, was blown into a Y-shape, and the two limbs of the Y drawn out as represented in fig. 3; a zinc rod was then cast so that a thin platinum wire was imbedded in one end; this zinc rod was brightened and

sealed up on the tube so that the platinum wire projected (fig. 4, *a*). By the aid of a glass funnel with a flexible capil-

Fig. 3. Fig. 4. Fig. 5.



lary stem (made by drawing out a piece of tubing before the blowpipe) paraffin-wax was introduced into the sealed-up limb so as to cover up completely the platinum wire and lower half of the zinc, leaving the other half exposed, *b*. In a similar way, recently boiled mercury was run into the other limb, previously sealed up with a second platinum wire passing through the glass, *c*. The stem of the inverted Y-tube was then carefully drawn out before the blowpipe (fig. 5, *m*), and connected by means of a short piece of india-rubber tubing, *n*, with the end of a glass tube projecting from the little flask, *p*, containing mercurous sulphate paste, and connected with the Sprengel pump. When a fairly good vacuum was obtained, the paste was made to boil by applying a very gentle heat; after about half an hour's boiling (the pump being at work the whole time) the connexion between the pump and flask was suddenly severed, when the sudden access of atmospheric pressure drove the paste into the cell, completely filling it; the pump was then again connected, and the boiling carried out again in the cell itself, and so on as before. Finally, by means of a blowpipe the drawn-out stem was sealed at *m*. To prevent the paste blocking up this drawn-out part, it was found necessary to use levigated particles of mercurous sulphate with a large proportion of zinc-sulphate solution; so that ultimately the cell contained much more fluid than solid matter. In order to use the cell, copper wires were soldered to the plati-

num wires projecting from the sealed ends of the inverted Y and bent over so as to dip into mercury-cups, the Y being either held by a clamp or imbedded in paraffin-wax, and of course being never allowed to be upset or shaken up so that the mercury could pass into the limb containing the zinc, which is otherwise liable to occur and spoil the cell.

136. The following numbers may be quoted as illustrations of the practical absence of any effect on the E.M.F. of the cell caused by the presence or otherwise of dissolved air. The values cited are the average readings, during a period of several months, of a dozen cells set up with cold-saturated zinc-sulphate solution well aerated, and *not* sealed up with melted paraffin-wax, but only loosely corked to avoid entrance of dust. Each cell during this period remained sensibly constant. All the values are reduced to the average reading (taken as 100·00), during the same period, of a yet larger number of cells prepared hot and sealed up precisely in accordance with Clark's directions—this average reading being the standard employed in the previous portions of these experiments, and especially in Parts V. and VI.

Batch of four cells made from mercurous sulphate purchased from Messrs. Hopkin and Williams	No. 1.	99·90
	„ 2.	99·94
	„ 3.	99·95
	„ 4.	99·99
Batch of four cells made from mercurous sulphate prepared specially by ourselves for this purpose	„ 5.	99·75
	„ 6.	99·95
	„ 7.	99·99
	„ 8.	100·02
Batch of four cells made from another specimen of mercurous sulphate prepared by ourselves	„ 9.	99·97
	„ 10.	100·11
	„ 11.	100·18
	„ 12.	100·19
General average		99·995

Precisely analogous figures were obtained with several vacuum-prepared cells, no one of which gave a value outside of the limits 99·7 and 100·3, *i. e.* outside of the limits of fluctuation of the ordinary Clark's cells compared with them. On opening one of these vacuum-cells so as to admit air, a distinct fall in E.M.F., amounting to 0·25 volt, was observed ;

this behaviour, however, was not shown by other similar cells on opening.

Influence of Mercuric Sulphate in the Mercurous Sulphate.

137. However carefully the mercurous sulphate may be prepared, it is almost impossible to obtain it without some admixture of mercuric sulphate. During the boiling and washing by decantation this latter becomes a basic salt, the so-called "turpeth mineral," which possesses a bright yellow tint, and communicates to the mercurous sulphate a more or less pronounced yellowish tinge. In order to see how far the presence of varying quantities of this compound might possibly affect the E.M.F. of Clark's cell, several cells were set up in the same way as the hot-prepared cells described above, but using turpeth mineral only instead of mercurous sulphate. Two samples of turpeth mineral were employed:—one purchased (from Messrs. Hopkin and Williams), and well washed by boiling up many times with water and decantation before use; the other prepared by boiling mercury with a large excess of pure sulphuric acid, evaporating off most of the acid (which process converts all mercurous sulphate present into mercuric), adding to a large bulk of boiling water and washing many times the yellow heavy powder formed, by boiling up with water and decanting, so as to remove all traces of free sulphuric acid, and of the soluble acid mercuric sulphate also formed. On taking a long series of readings of these cells, it was found that whilst the E.M.F. was, when the cell was newly set up, close to that of an average mercurous-sulphate cell, on standing a few days a distinct fall was observable, which went on progressively until, after some weeks, a diminution in the E.M.F. of between 3 and 4 per cent. was brought about, after which the fall ceased or became very languid. Thus the following average readings were obtained as before, the average E.M.F. of the hot-prepared cells containing saturated zinc-sulphate solution being taken as 100 when at the same temperature as the cells examined: cells A, B, C, and D were set up simultaneously with turpeth mineral prepared by ourselves; cells E and F simultaneously with the purchased substance. The zinc, zinc sulphate, and mercury used were the same as those used for the hot-prepared

cells. Notwithstanding, however, that all the cells were as alike as possible, yet the rate of fall during the first few weeks was by no means identical.

Age of cell...	1 day.	2 to 6 days.	1 to 2 weeks.	6 weeks.	2 to 4 months.	6 to 20 months.
Cell A	100·6	100·12	99·95	98·35	97·04	97·00
„ B	100·3	100·31	100·11	99·65	98·18	97·64
„ C	100·4	100·22	99·90	99·19	97·38	96·96
„ D	100·6	100·46	99·80	97·88	97·39	97·44
Average	100·5	100·28	99·93	98·77	97·50	97·26
Cell E	99·4	98·85	97·27	......	96·78	95·80
„ F	99·5	99·41	98·13	......	97·11	96·00
Average......	99·45	99·13	97·70	......	96·95	95·90

It is evident from these figures that the effect of the presence of turpeth mineral in the mercurous sulphate used for Clark's cells is in the direction of decreasing the value; but inasmuch as the decrease is progressive, whilst no such alteration was observed in the Clark's cells examined, at any rate during several months after construction, it appears doubtful whether the variations in the E.M.F. of different Clark's cells set-up at various times can be attributed to this cause.

Permanence of Clark's Cells.

138. A number of cells prepared in various ways (paste boiled and cells sealed with paraffin-wax; paste boiled *in vacuo* and cells hermetically sealed; set up with saturated zinc-sulphate solution, or with weaker solutions) were kept for periods of time ranging from a few months to two or three years, and checked against one another from time to time, or compared with Daniell cells set up as described in Part V., with amalgamated pure zinc and electro-copper plates, and pure zinc- and copper-sulphate solutions of the same molecular strength*.

* A large number of observations on the E.M.F. of Daniell cells have shown that, when proper precautions are taken in setting up the cells, a very considerable degree of constancy in value is attainable, so that such cells serve as good practical standards; but that if these precautions are neglected, *variations amounting to 5 per cent., and even more, may ensue.* The essential precautions are:—first, that pure solutions of zinc and copper sulphates containing no free acid should be used, each being of the same

No permanent changes in the values were observed (outside of the limit of the errors of observation) in the case of those cells which were so well sealed that neither evaporation took place, nor passage outwards of the fluid by capillary action through cracks in the sealing material. Vacuum-cells were thus kept unchanged for upwards of two years, as also were some normal Clark cells that were completely imbedded in paraffin-wax. In several cases, however, where the cells were not completely imbedded, but were only sealed up by a plug of paraffin-wax poured in at first round the zinc plate and the cork &c. supporting it, cracks formed sooner or later either in the paraffin-wax itself or between the glass and the wax, so that the fluid passed out through the cracks by capillary action and formed an efflorescence outside the cell. In some cases the action went on to such an extent as to leave the zinc wholly exposed, no contact finally existing between it and the paste: such cells were of course utterly spoilt. In other instances the zinc was only partially bared: in these cases the E.M.F. of the cell remained almost unaltered when saturated zinc-sulphate solution was employed in the first instance, but was lessened when unsaturated solution was originally used, owing to the evaporation and concentration which went on simultaneously with the capillary action, or subsequently to the commencement thereof. For example, two cells set up with zinc-sulphate solution about two thirds saturated gave the values 101·07 and 100·92 during the first few weeks

molecular strength (*i. e.* practically of the same specific gravity; conveniently the molecular strength may be near to MSO_4, $50H_2O$); secondly, that the solutions should be in separate vessels, united when required by an inverted U-tube, the mouths of which are covered with thin bladder (Raoult's form of cell); thirdly, that the plates should be pure zinc amalgamated with pure mercury, and copper recently electro-deposited from pure sulphate solution—the wires serving as electrodes, and their junctions with the plates being coated with gutta-percha, so that no part of the plate or wire is simultaneously in contact with both fluid and atmosphere; and fourthly, that, if used to generate a current, the current-density must not exceed some 5 microampères per square centimetre, so that with plates exposing 20 square centimetres the total resistance in circuit must be *at least* 10,000 ohms, if exposing 10 square centimetres 20,000 ohms, and so on.

after construction; cracks then formed, and efflorescence and evaporation took place, so that the zincs became partially bared, during which time the electromotive forces gradually sank. After some months the paste became covered with crystals of zinc sulphate, indicating that the residual solution moistening the mercurous sulphate was saturated: the electromotive forces were then 100·13 and 99·73 respectively, which values were subsequently retained almost constant for several months longer, notwithstanding that a considerable portion of each zinc rod was out of the paste and exposed to the air.

A number of observations made with cells containing zinc rods partly immersed in the paste and partly exposed to the air, gave sensibly the same average result as another series of observations made with the same cells when the zinc rods were pushed down so as to be wholly immersed (the upper end and the wire serving as electrode being protected from contact with the paste by gutta-percha).

It is specially to be noticed, in connexion with the question of the permanence of Clark's cells, that the cells experimented with were only used in connexion with the quadrant-electrometer; so that from first to last they *never generated any continuous current, nor had any current (however small) sent through in the inverse direction*—conditions impossible completely to realize in practice when working by the "method of opposition" or with the potentiometer.

Effect of Temperature on the E.M.F. of Clark's Cell.

139. According to Clark (Proc. Roy. Soc. xx. p. 444), the E.M.F. of a hot-prepared mercurous zinc-sulphate cell diminishes at an approximately constant rate of 0·06 per cent. per degree rise in temperature between 15°·5 and 100°; he states, however, that this figure might be verified with advantage. A number of observations having indicated, as a preliminary result, that this value is considerably too high between the temperature-range (10° to 25°) most frequently obtaining in practice, and that fairly constant results are given with different cells, the following experiments were made in order to determine more exactly the mean coefficient of alteration per

degree between these temperature-limits, with the result of showing that, instead of Clark's number (0·0006) being deduced, a value but little above two thirds of this figure was obtained, viz. ·000411, as the average of ten experiments with five cells.

Let the E.M.F. of a given cell, taken temporarily as a standard, be 1 at temperature t_1 (near to 15°), and let the E.M.F. of a second cell compared therewith be a_1 when the cell compared is at a temperature t_2, the standard being still at t_1. In another experiment, when the standard is at a temperature t_3 not far from t_1, let the E.M.F. of the second cell be a_2, this cell being at the temperature t_4. Now let x be the mean coefficient of variation for 1° between t_2 and t_4 for the second cell, whilst x' is the analogous coefficient between t_1 and t_3 for the temporary standard. Then, since the E.M.F. of the standard at t_1 is unity, its E.M.F. at t_3 is $1-(t_3-t_1)x'$, whence the E.M.F. of the second cell at t_4 is $a_2\{1-(t_3-t_1)x'\}$. The E.M.F. of this second cell at t_4, however, is also

$$a_1\{1-(t_4-t_2)x\};$$

so that

$$a_1\{1-(t_4-t_1)x\}=a_2\{1-(t_3-t_1)x'\}.$$

Now, from Clark's experiments and certain preliminary observations made by ourselves, it results that x is approximately equal to x'; whilst if the temperatures are suitably chosen so that the mean of t_1 and t_3 is sensibly the same as the mean of t_2 and t_4, it must result that the difference between x and x' is very small; and, finally, if t_1 and t_3 differ but little in comparison with the difference between t_2 and t_4, any errors in the valuation of x' will be but small relatively. Hence, taking $x=x'$, it results that

$$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}.$$

In order, then, to determine x, it is only necessary to determine the relative readings of two cells, first when one is at t_1 and the other at t_2 (say at 15° and 0° respectively), and secondly when the first is at t_3 and the second at t_4 (say at

14° and 30° respectively), the temperatures being such that t_1+t_3 approximately equals t_2+t_4 (as in the case of the supposed numbers).

To carry out this principle two water-jacketed metal chambers were constructed, furnished with delicate thermometers reading to 0°·01 C., and containing respectively the sets of cells to be compared, the poles of the cells being connected with the mercury-cup arrangement applied to the electrometer by means of covered wires passing through narrow glass tubes fixed in the double lids of the chambers, so that no conducting contact between the wires themselves or between the wires and lids &c. was possible. One of the water-jackets was filled with water at near 15°, the other with water either at or near 0° or at or near 30°, as the case might be; the masses of fluid (agitated from time to time with a peculiar stirrer) were so large that the temperature of the chamber-spaces varied but little during the progress of the series of readings ultimately made. The mean temperatures indicated by the thermometers during the series were taken as the mean temperatures of the cells (placed in the chambers some time before the readings were commenced, so as to attain sensibly the temperatures of the chamber-spaces). The readings were carried out in systematic order; so that the average reading for each cell should be exactly comparable with that of any other, notwithstanding any possible running-down of the electrometer-scale during the progress of the readings. For instance, if in the first chamber two cells (A and B) were placed, and in the second two others (C and D), the readings were alternately taken in the orders A, B, C, D and D, C, B, A, or C, D, A, B and B, A, D, C; so that the average reading for each cell was identical with that which would have been observed had the electrometer-scale value been absolutely constant throughout at its mean value (the actual variation of the electrometer-scale during any set of readings was considerably under 1 per cent.).

Thus, for instance, the following numbers were obtained in two experiments, in each of which the same two cells A and B were placed in the first chamber, and the same two (C and D) in the second:—

	1st experiment.	2nd experiment
t_1	$16^\circ{\cdot}84$	$17^\circ{\cdot}04$
t_2	$1^\circ{\cdot}08$	$3^\circ{\cdot}30$
t_3	$9^\circ{\cdot}72$	$10^\circ{\cdot}98$
t_4	$26^\circ{\cdot}02$	$25^\circ{\cdot}12$
Average scale-reading for A and B taken together at t_1	159·94	159·62
Average reading for C at t_2	161·00	160·75
Average reading for A and B taken together at t_3 .	153·00	152·56
Average reading of C at t_4	152·25	152·00
a_1 . . .	$\frac{161}{159{\cdot}94}=1{\cdot}0066$	$\frac{160{\cdot}75}{159{\cdot}62}=1{\cdot}0071$
a_2 . . .	$\frac{152{\cdot}25}{153}= {\cdot}9951$	$\frac{152}{152{\cdot}56}= {\cdot}9963$
$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}$ . . .	·000358	·000386

Mean value of x = ·000372.

Similarly the values ·000439 and ·000428 (mean = ·000434) were obtained for x in the case of cell D simultaneously examined. The following Table exhibits in brief these figures and those obtained in six other experiments with three other different cells:—

	1st experiment.	2nd experiment.	Mean.
1st cell	·000358	·000386	·000372
2nd „	·000439	·000428	·000434
3rd „	·000480	·000481	·000481
4th „	·000436	·000397	·000417
5th „	·000364	·000336	·000350
		General average	·000411

Hence, finally, it results that the E.M.F. of a Clark's cell set up with saturated zinc-sulphate solution is, at a temperature t not more than 10° or 12° above or below $15^\circ{\cdot}5$ C.,

$$1{\cdot}457\{1-(t-15^\circ{\cdot}5)\times 0{\cdot}00041\} \text{ volt};$$

it being admitted that Clark's valuation is exact, viz. 1·457 volt at $15^\circ{\cdot}5$.

On the Work done during Electrolysis.

140. The experiments described in the previous portions of these researches have shown that, when a current is passed through an electrolytic cell, the amount of energy expended (positively or negatively) during the passage in performing a given amount of chemical work (apart from that transformed into heat in consequence of the resistance proper of the cell in accordance with Joule's law) is not constant, but *increases algebraically with the current-density*, in such wise that when the cell is an ordinary decomposing cell (*e. g.* a voltameter) the "counter electromotive force" of the cell increases in arithmetical value with the current-density, whilst when the cells is an electromotor (*i. e.* such a cell as to yield a *negative* counter E.M.F.), the arithmetical value of the negative counter E.M.F. (*i. e.* the direct E.M.F. of the cell) decreases with the current-density. The extra work done by a stronger current as compared with a weaker one in the former case, and the deficiency of work corresponding with the fall in direct E.M.F. in the latter case, make their appearance in the form of sensible heat in the cell.

Experiments have been published by Favre (*vide* Part I. §§ 14 and 15) which appear to show that certain forms of electromotor-cells can generate currents capable of doing more work externally to the cell than corresponds with the net chemical action taking place, this extra work being gained at the expense of the sensible heat of the cell, which becomes *cooled* by the passage of a current of too small magnitude to generate, in accordance with Joule's law, sufficient heat in the cell to overpower this cooling action. Inasmuch, however, as the mercury-calorimeter was employed in these experiments of Favre, whilst, from the nature of the case, but feeble currents passed, so that the total amount of chemical action in a given time could be but small, it seems not unlikely that an excessively large probable error attends the numerical values obtained. In point of fact, one of the cells found by Favre to behave in this way was Grove's cell; and his results in this repect are totally at variance with all other experiments on the subject (compare H. F. Weber, Phil. Mag. 1878, v. p. 195), leading to the conclusion that the supposed cooling action was

not a real effect, but simply the result of the accumulation of experimental errors. In order to see whether this was also the case with the other cells examined by Favre, the following experiments were made.

These other cells were simple voltaic couples of zinc and platinum or cadmium and platinum immersed in dilute hydrochloric acid; the numbers obtained by Favre as the cooling effects per gramme equivalent of metal dissolved were respectively 1051 and 1288 gramme-degrees, corresponding with ·046 and ·057 volt*. On the other hand, with dilute sulphuric acid in lieu of hydrochloric, Favre found that no cooling action was traceable, but that the cells were always warmed by the passage of a current. Now these results, if correct, must imply that the E.M.F. of a zinc-platinum or a cadmium-platinum cell, when generating only a minute current, is above the value corresponding with the heat-development due to the net chemical action taking place when hydrochloric-acid solution is the exciting fluid, and below that value when dilute sulphuric acid is used instead; *i. e.* the electromotive forces of cells containing dilute hydrochloric acid must be above ·754 and ·388 volt respectively with zinc-platinum and cadmium-platinum couples, and the electromotive forces of cells containing dilute sulphuric acid must be below ·835 and ·470 volt respectively with these same couples, these being the values in E.M.F. corresponding respectively with the heat-developments per gramme-equivalent in the reactions

$$\begin{aligned} Zn + 2HCl &= ZnCl_2 + H_2, \\ Cd + 2HCl &= CdCl_2 + H_2, \\ Zn + H_2SO_4 &= ZnSO_4 + H_2, \\ Cd + H_2SO_4 &= CdSO_4 + H_2, \end{aligned}$$

these heat-developments being, per gramme-molecule, as follows†:—

* For the sake of comparison with the experiments described in the previous portions of these researches, the factor 4410 for converting gramme-degrees into volts is adhered to, notwithstanding that the balance of evidence now seems to indicate that the value of J hitherto assumed (42 megalergs) is somewhat too high, and that the B.A. unit of resistance is upwards of 1 per cent. below its intended value, instead of being exact as hitherto assumed.

† These figures are deduced from Julius Thomsen's thermochemical

Zn, Cl_2, aq. . . .	$=112840$	Cd, Cl_2, aq. . . .	$=96250$
H_2, Cl_2, aq. . . .	$=78640$	H_2, Cl_2, aq. . . .	$=78640$
Difference . .	34200	Difference . .	17610
Diff. per gramme-equivalent	17100	Diff. per gramme-equivalent	8805
Corresponding with volt	·754	Corresponding with volt	·388
Zn, O, SO_3, aq. . .	$=106090$	Cd, O, SO_3, aq. . .	$=89500$
H_2, O	$=68200$	H_2, O	$=68200$
Difference . .	37890	Difference . .	21300
Diff. per gramme-equivalent	18945	Diff. per gramme-equivalent	10650
Corresponding with volt	·835	Corresponding with volt	·470

141. In order to see whether the electromotive forces actually developed by these four voltaic combinations are really above the calculated values in the first two cases and below in the second two instances, when the disturbing effects of dissolved air are eliminated, cells were set up like those described in § 85, and caused to generate feeble currents by employing large external resistances. In all cases it was found that when the errors due to dissolved air were eliminated and the readings became constant, the E.M.F. actually developed *invariably fell short of the value corresponding with the net chemical action* by an amount which increased with the current-density until the reduction became a large fraction of the E.M.F. observed with the smallest possible densities. With hydrochloric-acid cells the deficiency was not so great in the first instance, and the rate of increase in deficiency was not so rapid, as with sulphuric-acid cells. Thus the following four experiments may be cited as illustrations of the results obtained in numerous cases:—

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data and the mean value for the heat of formation of water arrived at in § 31. Thomsen's values relate to the degree of dilution $MCl_2, 400\,H_2O$, and $MSO_4, 400\,H_2O$. Some experiments made by us on the amounts of heat evolved on diluting stronger solutions of zinc and cadmium chlorides and sulphates indicate that these values require slight corrections for stronger solutions than those used by Thomsen; but the alterations thus produced in the net heat-development and in the E.M.F. corresponding thereto is but small.

Hydrochloric Acid: Zinc and Platinum.

Current, in microampères, =C.	Current-density, in microampères, per square centimetre.	Observed differences of potential between plates =E.	Value of CR.	E.M.F. of cell, e=E+CR.
12·6	1·6	·633	......	·633
23·4	2·9	·628	·001	·629
55·1	6·9	·609	·002	·611
102·4	12·8	·585	·003	·588
224·5	28·1	·545	·007	·552

Calculated E.M.F. .... = ·754

Hydrochloric Acid: Cadmium and Platinum.

6·5	0·8	·347	......	·347
11·0	1·4	·291	·001	·292
14·8	1·85	·249	·001	·250
33·7	4·2	·161	·003	·164
54·3	6·8	·130	·005	·135
97·8	12·2	·103	·010	·113

Calculated E.M.F. .... = ·388

Sulphuric Acid: Zinc and Platinum.

12·6	1·6	·626	......	·626
23·4	2·9	·540	·001	·541
55·1	6·9	·492	·002	·494
102·4	12·8	·439	·003	·442
224·5	28·1	·353	·007	·360

Calculated E.M.F. .... = ·835

Sulphuric Acid: Cadmium and Platinum.

6·5	0·8	·301	......	·301
11·0	1·4	·259	·001	·260
14·8	1·85	·211	·001	·212
33·7	4·2	·080	·003	·083
54·3	6·8	·033	·005	·038
97·8	12·2	·019	·010	·029

Calculated E.M.F. = ... ·470

In each of these experiments the plate-surface was constantly 8 square centimetres; the hydrochloric-acid solution was close to $2\,HCl, 50\,H_2O$ and $2\,HCl, 100\,H_2O$ in the first and second experiments respectively, and the sulphuric-acid $H_2SO_4, 50\,H_2O$ and $H_2SO_4, 100\,H_2O$ in the third and fourth

experiments respectively. The zinc plates were amalgamated, the cadmium ones not.

142. The experiments described in Parts IV., V., and VI. indicate that the amount of diminution brought about in the E.M.F. of an electromotor (either a simple cell, or one after Daniell's construction) by an increase in the current-density may readily greatly exceed any possible effect due to the accumulation round the two plates of fluids of widely different molecular strength, and, further, that, as a general rule, the effect of diminishing the area of the plate on which the metal is deposited is considerably greater than that of a similar diminution in the area of the other plate, although this is not invariably the case. It is hence evident that the chief source of nonadjuvancy especially lies in the incomplete manifestation as electricity of the energy due, after the elimination by the action of the current of the deposited metal (or body equivalent thereto) in the nascent form, to the subsequent transformation thereof into the permanent form. Clearly the same kind of thing must be equally true for the other products of electrolysis evolved at the other electrode. Hence the reason why a less amount of non-adjuvancy is brought about at this side is presumably the greater amount of attraction exercised by the material of the electrode for the nascent product ("sulphion" of Daniell in the case of cells containing sulphates) here evolved, owing to their opposite chemical characters, than is observable at the other electrode. Admitting this to be so, it should result that the more oxidizable the metal dissolved (*i. e.* the greater the heat of formation of the compound produced by its solution), the less will be the amount of nonadjuvancy due to the incomplete conversion into electricity at this plate of the energy due to transformation of nascent into final products. The results of the experiments hitherto described, however, being complicated by the formation of solutions of different strengths around the two plates, are not sufficiently precise to show that, under given conditions, a zinc plate, for example, causes less nonadjuvancy than a cadmium one, and so on. Accordingly the following experiments on the point were made, the result of which is to show indisputably that the more oxidizable the metal the less the nonadjuvancy.

An electrolytic cell was constructed, consisting of a wide glass tube closed by india-rubber bungs through which passed wires terminating interiorly in the plates to be experimented with, the opposed plate-surfaces being perpendicular to the axis of the tube and therefore parallel to one another, and the anterior portions of the plates and the wires being thickly coated with gutta-percha. The tube was then filled, for instance, with concentrated zinc-sulphate solution, with plates of zinc at an accurately known distance apart, and was kept at a temperature sensibly uniform. A series of currents of various strengths was then passed through the cell, and the difference of potential subsisting between the plates determined in each case. These values represented the numerical values of $e_1 + \mathrm{CR}$, where e_1 is the counter E.M.F. set up during the electrolysis, C the current, and R the resistance of the cell; and from them the values of this expression for definite values of C (50, 100, 200 microampères, &c.) were readily calculated by interpolation. The + zinc electrode was then removed, and a copper plate exposing exactly the same area placed in precisely the same position. The observations were then repeated, the temperature being the same as before, and a new series of values, $e_2 + \mathrm{CR}$, calculated, e_2 being the counter E.M.F. now set up for a given value of C. Since R is constant throughout, it is evident that the difference between the two values for a given current obtained, first with a zinc, and secondly with a copper + electrode, represents $e_2 - e_1$. Now necessarily both e_1 and e_2 increase with the value of C in accordance with the general law to that effect deduced from all the previous observations (§ 133); but if it be true that a less production of heat instead of electricity is brought about when nascent sulphion is liberated in contact with zinc than when in contact with copper, e_1 must increase less rapidly with the current than e_2, and hence the value of $e_2 - e_1$ *must rise with the current-strength.* Precisely this result was observed in every case: for example, the following numbers were obtained in a pair of sets of observations carried out as described, the area of the plates being 0·50 square centim. throughout.

+ Zinc electrode.		+ Copper electrode.	
C.G.S. current.	Observed potential-difference.	C.G.S. current.	Observed potential-difference.
·00000436	·018	00000466	1·073
·00000866	·039	00000883	1·089
·00001432	·062	00001460	1·127
·00002130	·091	00002275	1·159
·00004160	·174	00004450	1·251

From these figures the following are obtained by interpolation:—

Current.	Potential-difference.		
	+ Zinc.	+ Copper.	e_2-e_1.
·000005	·021	1·075	1·054
·00001	·045	1·101	1·056
·00002	·085	1·147	1·062
·00004	·168	1·232	1·064

Precisely similar results were obtained in numerous other analogous experiments. Thus the following Table illustrates some of the figures obtained, the + zinc plate originally employed being replaced by a plate of the same size, I. of bright copper, II. of electro-copper, III. of amalgamated copper, IV. of bright cadmium, V. of bright silver.

C.G.S. current.	Values of e_2-e_1 obtained				
	I.	II.	III.	IV.	V.
·000005	1·063	1·054	1·065	......	1·486
·00001	1·067	1·057	1·076	......	1·498
·00002	1·073	1·061	1·084	·315	1·503
·00004	1·075	1·068	1·099	·324	1·512

143. A still better illustration of the regular rise in value of e_2-e_1 with the current is afforded by the following series of numbers obtained as the average results of several sets of observations very carefully made—A with a bright zinc +

electrode, B with one of bright cadmium, C with one of bright copper, and D with one of bright silver. In every case the mean temperature was the same within two or three tenths of a degree (varying from 17°·55 to 17°·9 throughout). In the last case it was found that, whilst perfectly steady readings could be obtained with current-strengths up to something like ·0007, with higher strengths this was no longer the case, silver peroxide being apparently formed instead of silver sulphate. In these experiments all the plates exposed an area of 1·5 square centim., the solution electrolyzed being a nearly saturated one of pure zinc sulphate, renewed for each series; the plates were about 5 centim. apart, the tube holding them being 3 centim. in internal diameter.

C.G.S. current.	Difference of potential set up.			
	A.	B.	C.	D.
·00002	·029	·317	1·069	1·490
·00005	·044	·334	1·086	1·509
·0001	·063	·354	1·107	1·530
·0002	·084	·381	1·139	1·562
·0005	·146	·451	1·210	1·636
·001	·230	·547	1·310	
·0015	·311	·636	1·403	
·002	·389	·730	1·498	
·0025	·476	·830	1·598	

These figures yield the following six sets of values of e_2-e_1 for the corresponding pairs of + electrodes compared.

Current.	Zinc-cadmium.	Zinc-copper.	Zinc-silver.	Cadmium-copper.	Cadmium-silver.	Copper-silver.
·00002	·288	1·040	1·461	·752	1·173	·421
·00005	·290	1·042	1·465	·752	1·175	·423
·0001	·291	1·044	1·467	·753	1·176	·423
·0002	·297	1·055	1·478	·758	1·181	·423
·0005	·305	1·064	1·490	·759	1·185	·426
·001	·317	1·080	......	·763		
·0015	·325	1·092	......	·767		
·002	·341	1·109	......	·768		
·0025	·354	1·122	......	·768		

Not only does the value of e_2-e_1 increase with the current-density in every case, but, further, the rate of increase is greater when zinc is compared with silver than with copper, and greater then than when compared with cadmium; similarly the rate of increase with cadmium and silver is greater

than with copper and silver, and so on. It is further noticeable that in each case a value of $e_2 - e_1$ with some particular current-strength is deducible which is sensibly the same as the E.M.F. of a cell after Daniell's construction containing the same metals and sulphate solutions of equal molecular strength ; so that in general it may be said that, for a current-density below a particular limit, the value of $e_2 - e_1$ is less than that of the corresponding Daniell form of cell, whilst for a current-density above this limit it is greater.

144. The following experiment seems to show that the substitution of dilute sulphuric acid for zinc-sulphate solution as the electrolyte makes no material difference in the end result, the — electrode being made of platinum, and the disturbing influence of dissolved air being eliminated. Two precisely similar U-tube cells (§ 85) were filled with recently boiled dilute sulphuric acid (11·5 grammes H_2SO_4 per 100 cubic centim.), and fitted with uniformly sized plates (8 square centim. total surface in each case) at an equal distance asunder, so that the resistance of the cell should be sensibly the same in each case. In the first cell the plates were of zinc (amalgamated) and platinum, and in the second of copper and platinum respectively; the two were arranged in series with a couple of Leclanché cells, so that the platinum plates were necessarily the — electrodes; a large variable resistance being included in the circuit, the current could be regulated at pleasure. A current of some fifty microampères being sent through for three days, the readings became steady when all the dissolved air around the platinum plates was eliminated; the current was then varied from time to time, and a series of readings of the potential-difference between each pair of plates taken. By interpolation as before, the following figures were then deduced from the average values.

Current in micro-ampères.	Micro-ampères per square centim.	Difference of potential.		$e_2 - e_1$.
		+ zinc.	+ copper.	
20	2·5	−·552	+·449	+1·001
40	5·0	−·558	+·448	+1·006
80	10·0	−·498	+·521	+1·019

The value of e_2-e_1 consequently increases with the current-density as before. The numerical values observed in this experiment are somewhat lower than those found in the experiments above described, as might be expected, since the largest current-density employed in this case, being only 10 microampères per square centim., is considerably below the smallest cited in the previous observations, in the last of which a minimum current of ·00002 C.G.S. units (or 200 microampères) was employed with plate-surfaces of 1·5 square centim., giving a density of 133·3 microampères per square centim., in which case the value of e_2-e_1 was 1·040; whilst in the former experiments a minimum current of ·000005 C.G.S. unit (50 microampères) was employed with a plate-surface of ·50 square centim., giving a density of 100 microampères per square centim., when values of from 1·054 to 1·065 were observed.

145. Some experiments were also made with analogous pairs of cells in which the + electrodes were made of metals not attacked by the nascent products arising from the electrolysis of sulphates, *e. g.* gold and platinum. In these instances it was found that platinum behaved in reference to gold just as a more readily to a less readily oxidizable metal, this result being evidently brought about by the superior surface condensing-power possessed by platinum, in virtue of which a greater proportion of the energy due to the transformation of the nascent into the final products of electrolysis evolved at the + electrode becomes adjuvant. For instance, the following numbers were obtained with a pair of precisely similar cells containing the same copper-sulphate solution and copper − electrodes.

Current-density, microampères per square centimetre.	Difference of potential between plates.		Difference.
	+Platinum.	+Gold.	
3·0	1·500	1·555	·055
7·0	1·534	1·591	·057
11·0	1·570	1·630	·060

Even with the lowest current-density and with platinum as

+ electrode the total amount of nonadjuvancy was here considerable; for the E.M.F. corresponding with the net chemical action is only 1·234 volt ($\frac{1}{2}$[Cu, O, SO_3 aq] = 27,980 gramme-degrees = 1·234 volt); and the minimum difference of potential set up, after correction for the resistance of the cell (*i. e.* the counter E.M.F. set up, or the value of the term e in the expression $E = e + CR$), exceeds 1·490, since the term CR in this case was much less than ·010 volt.

In just the same kind of way, when platinum and gold were respectively made the — electrodes in similar pairs of cells containing dilute sulphuric acid and a constant oxidizable + electrode, the superior surface condensing-power possessed by platinum caused a less degree of nonadjuvancy during the transformation of nascent into free hydrogen. Thus, for example, the following numbers were obtained with a copper + electrode and acid containing 10 per cent. of H_2SO_4.

Current-density, micro-ampères per square centimetre.	Difference of potential between plates.		Difference.
	—Platinum.	—Gold.	
2·5	·449	·575	·126
5·0	·488	·619	·131
10·0	·521	·661	·140

Here again, even in the most favourable instance, with the smallest current-density and platinum as — electrode, a conderable amount of nonadjuvancy subsisted; for the value of CR in this case was not greater than ·001; so that the minimum counter E.M.F. set up was at least ·448 volt, whilst the E.M.F. corresponding to the net chemical action is only ·270 volt, the heat-development being per gramme equivalent

$\frac{1}{2}(H_2, O)$ . . = 34100 gramme-degrees.
$\frac{1}{2}(Cu, O, SO_3\,aq)$ = 27980 " "

6120 " " =·270 volt.

It is hence evident, *à fortiori*, that when acidulated water is decomposed with two gold electrodes, the counter E.M.F. set up must be much greater for a given current-density than

when two platinum electrodes are used, the deficiency in condensing-power being then manifest at both electrodes simultaneously. The experiments described in Part IV. § 90 have shown that this is the case.

146. In addition to the experiments above described as examples, a large number of analogous observations have been made with varying kinds of electrolytic solutions and electrodes, and with varying strengths of solutions. The general results of these experiments, so far as at present completed, may be thus summarized.

(1) When an electrolytic cell is of such a nature that the counter E.M.F. set up is negative (*i. e.* when the cell is an electromotor), it is always found that *the E.M.F. developed is less the greater the density of the current generated.* With very small current-densities the E.M.F. has a maximum value which in certain cases (*e. g.* Daniell's cell and the analogous zinc-cadmium and cadmium-copper cells described in Part VI.) is substantially identical with the E.M.F. corresponding with the heat-development due to the net chemical action taking place in the cell, *i. e.* with the E.M.F. representing the algebraic sum of the chemical affinities involved. In certain other cases (*e. g.* the zinc-silver, cadmium-silver, and copper-silver cells described in Part VI.) the maximum E.M.F. developed is sensibly *below* that due to the net chemical action.

(2) Some kinds of combinations have been found to be capable of existing which can develop a *greater* E.M.F. than that due to the net chemical action (although the particular cells described by Favre as possessing this property are not really cases in point, Favre's results being due to experimental errors); amongst such combinations may be mentioned several where *lead* is the metal dissolved, *i. e.* lead-copper cells charged with solutions of acetates. It is noticeable that in such cases Volta's law of summation holds, the sum of the electromotive forces of two cells, one containing zinc and lead and the other lead and copper, being equal to the E.M.F. of a zinc-copper cell, the E.M.F. of the first cell being just as much below the amount calculated from the heat-development as that of the second is above the amount similarly calculated. This class of cells is now undergoing careful examination, and will be dealt with in a subsequent paper. Unfortunately,

progress in this direction during the last fifteen months has been greatly retarded by the refusal of the Administrators of the Government Fund of £4000 to continue the grants by the aid of which the previous portions of these researches have mainly been made, on account of which circumstance numerous other points of interest that have cropped up have necessarily remained uninvestigated *.

(3) When the electrolytic cell is not an electromotor, the counter E.M.F. set up (positive) always *increases in amount with the current-density.* When the + electrode is of such a nature as to combine with the products of electrolysis evolved thereat, other things being the same, *the rate of increase is slower the greater the chemical affinity* between the nascent products of electrolysis evolved at the + electrode and the material of which that electrode is composed; *i. e.* the greater the affinity, the less the degree of nonadjuvancy brought about at the + electrode.

(4) Whether the cell be an electromotor or not, there is always (with currents not so small as to be practically infinitesimal) a greater or less degree of nonadjuvancy brought about at the — electrode, owing to the development of heat in lieu of electricity during the transformation of nascent into ultimate permanent products of electrolysis. In many cases this source of nonadjuvancy decidedly predominates over that at the + electrode.

(5) The particular extent to which the nonadjuvancy reaches at either electrode appears to be a complex function not only of the chemical nature of the electrode, the physical conditions of its surface, and the character of the nascent products of electrolysis evolved thereat, but also of the temperature, and the degree of concentration of the solution electrolyzed, and possibly of other conditions besides. Other things being equal, it appears to be a general rule that *the weaker the solution, the greater the degree of nonadjuvancy.* When a gas is

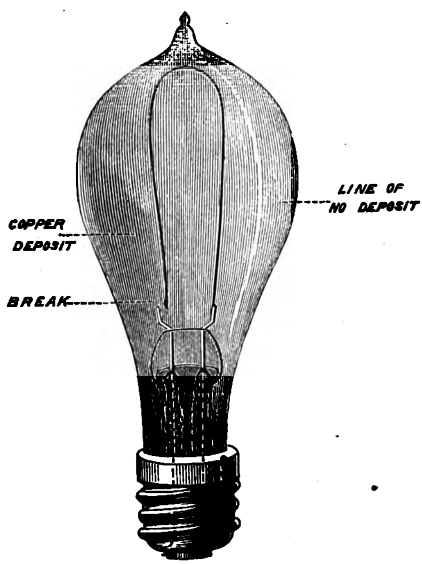
* Since the presentation to the Physical Society of Part VI. of these researches, a paper has appeared by F. Braun (*Annalen der Phys. u. Chem.* xvi. p. 561), in which the author shows that various combinations examined by him give electromotive forces sensibly the same as those calculated from thermochemical data, whilst others fall short of, and some exceed, the calculated values.

one of the permanent products of electrolysis at either electrode, *the greater the surface condensing-power of the material of which the electrode is composed, the less is the degree of non-adjuvancy.*

XXXI. *On a Phenomenon of Molecular Radiation in Incandescence Lamps.* *By* J. A. Fleming, *B.A., D.Sc.**

Not long ago a curious phenomenon came under my notice in connexion with the burning of Edison incandescence lamps, which presents sufficient interest to warrant my drawing the attention of physicists to it.

As is well known, the carbon filament in the Edison lamp is of a horse-shoe form. The two extremities of the loop are



clamped into small copper clamps on the ends of the platinum wires, which are sealed through the glass. The ends of the carbon loop are electroplated over with copper at the place where they are connected to the clamp in order to make a good contact. If this precaution is omitted, a loose contact

* Read May 26, 1883.

may be formed, the result of which will be a generation of heat at that point.

In the ordinary working the life-history of a carbon filament is something as follows:—

At some point or other the filament is probably thinner than at other places. At this place there will be a greater generation of heat and a higher temperature; volatilization of the carbon ensues, and the vapour condenses on the sides of the glass bulb, as far as I have observed, uniformly. If, however, the point of greatest resistance occurs on the copper clamp, then it is found that copper volatilizes and deposits on the inside of the glass.

But what is most curious is, that in this case an examination of the glass envelope shows that there is a narrow line along which no copper has been deposited. This is seen best by holding the lamp up before the light and slowly turning it round. In one particular position, easily found, it is best seen. Now, on examining carefully the position of the line of no deposit as compared with the position of the carbon filament, it will be seen that it lies in the plane of the loop, and on the opposite side to that nearest to which the break of the loop has occurred. It is in fact *a shadow of the loop.*

The conclusion which must be arrived at, then, is that the copper molecules are shot off in straight lines; otherwise it is impossible that there should be this line of no deposit.

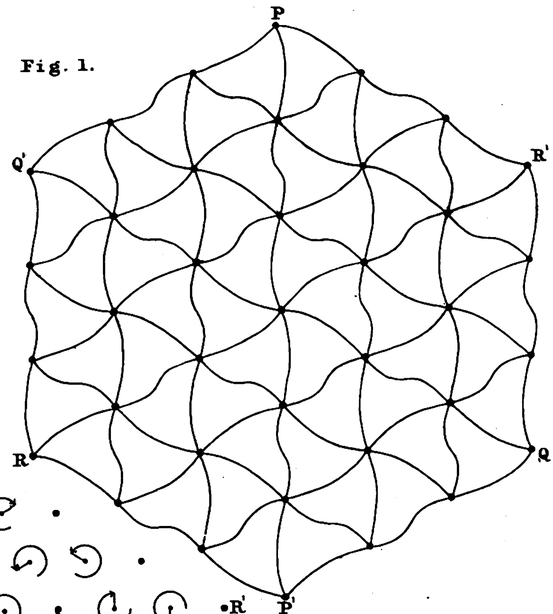
The most noticeable thing is, that it occurs only when the deposition of copper takes place; I have never noticed it in an ordinary carbon deposit.

Hence there must be some essential difference between the vaporization of the carbon and that of the copper. The carbon deposit resembles more the condensation of a vapour and is uniformly distributed; but the copper deposit exhibits the character of a molecular radiation or shower taking place from a certain point.

The whole phenomenon calls at once to mind the beautiful researches of Mr. Crookes with vacuum-tubes. Here, however, we are dealing not with an induction-coil discharge, but with a comparatively low potential.

I have never failed to see the effect in any lamp which has had a deposition of copper on its interior.

Fig. 1.



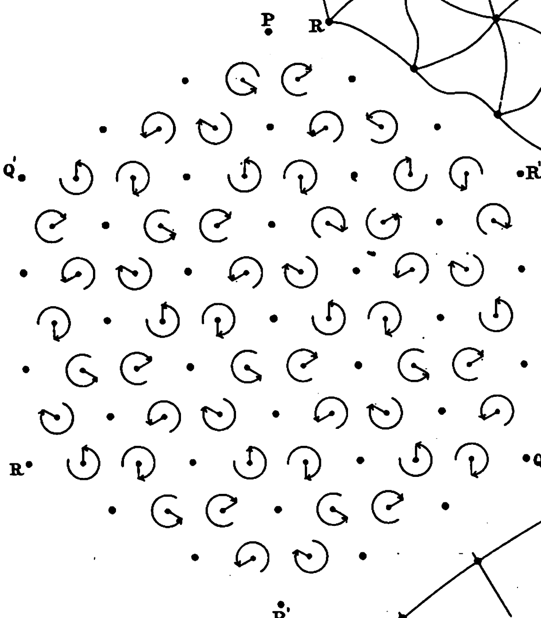


Fig. 2.

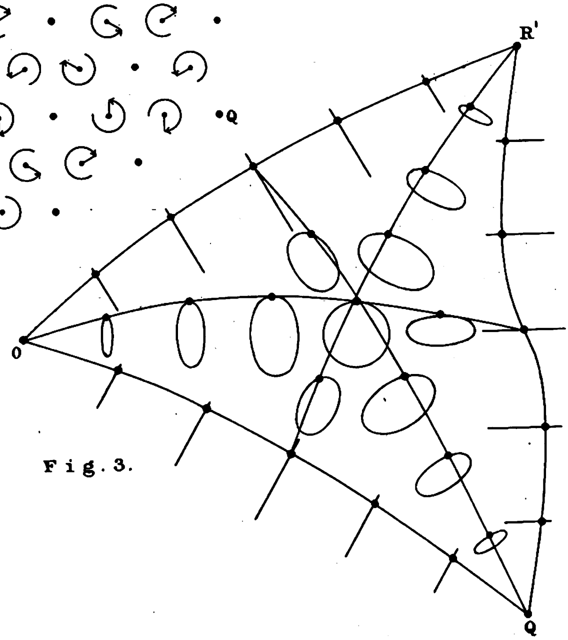


Fig. 3.

It is interesting to note how nearly the colour of transparent copper resembles that of transparent gold. The similarity of the surface-colour of pure unoxidized copper and of gold is accompanied by a near resemblance in colour of the two metals in thin films.

XXXII. *An Illustration of the Crossing of Rays.* *By* WALTER BAILY*.

[Plate XII.]

WHEN rays of light are passing through a point, the resultant motion of the æther is in general far too complicated to be conceived; but if the light is homogeneous, it can readily be shown that the motion at each point is simply harmonic motion in an ellipse; so that in that case the complication consists only of the change in this ellipse in passing from one point to another. Hence a model might be constructed to represent the crossing of homogeneous rays by placing a number of ellipses to represent the motion at a number of separate points, through which the light might be supposed to be passing. If we further simplify the case by considering only rays parallel to one plane, and suppose them to be plane-polarized so that the vibrations are parallel to the same plane, the whole motion will be parallel to that plane, and might be represented by means of diagrams.

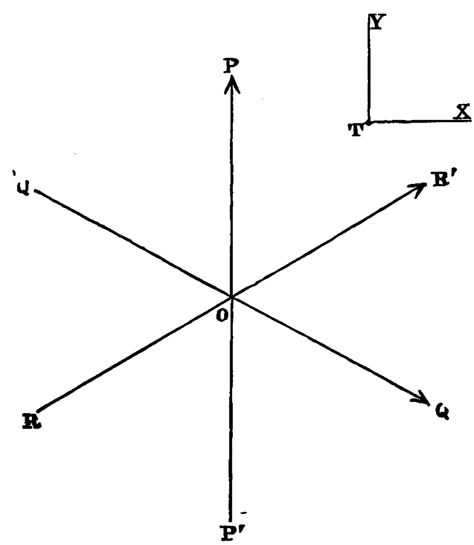
The case worked out in this paper is that of three rays of equal intensity parallel to one plane, plane-polarized so that the vibrations are parallel to that plane, and meeting one another at equal angles.

Take any point O, and let P′ O P, Q′ O Q, R′ O R be the rays through O. Take any other point T in the same plane; draw T X, T Y perpendicular and parallel respectively to P′ O P. Let p, q, r be the distances from O of the feet of the perpendiculars drawn from T on P′ O P, Q′ O Q, R′ O R respectively; these distances being considered positive if drawn towards P, Q, R, and negative if drawn towards P′, Q′, R′. Then it may be shown that

$$p+q+r=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* Read May 26, 1883.

The position of T may be defined by any two of these quantities. The equations $p=$const., $q=$const., $r=$const., are equa-



tions to straight lines perpendicular to P′ O P, Q′ O Q, R′ O R respectively; and the equations $q-r=$const, $r-p=$const., $p-q=$const. are equations to lines parallel to P′ O P, Q′ O Q, R′ O R respectively. When the constant is zero, the lines pass through O.

If we take any point in Q′Q and move perpendicularly to Q′ Q from this point, we can, without altering the phase of the vibration of the ray Q, reach a point at which the phase of the vibration of the ray R is the same. If we now move from this latter point in a direction parallel to P′ P, we shall keep the phases of Q, R equal to one another, and we can reach a point at which the phase of the ray P is equal to either of them. Take this point as the origin, and let the phases be zero at the initial time. Then at a time t the displacements due to the three rays at the point T will be $\sin 2\pi(t-p)$, $\sin 2\pi(t-q)$, $\sin 2\pi(t-r)$, the wave-length being taken as the unit of length, and the period as the unit of time.

Let x be the amount of displacement along TX and y that along TY, at the time t. Then

$$x = \sin\frac{\pi}{2}\sin 2\pi(t-p) + \sin\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q) + \sin\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r),$$

$$y = \cos\frac{\pi}{2}\sin 2\pi(t-p) + \cos\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q) + \cos\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r).$$

By means of (1) these equations may be written

$$x = \sin 2\pi(t-p) - \cos\pi(q-r)\sin 2\pi\left(t+\frac{p}{2}\right), \quad . \quad (2)$$

$$y = \sqrt{3}\sin\pi(q-r)\cos 2\pi\left(t+\frac{p}{2}\right). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In general the calculation of the phase and the ellipse would be laborious; but it may be readily effected along lines parallel to P′OP, Q′OQ, R′OR at distances $\frac{1}{\sqrt{3}}$ from one another as follows:—We have as equation to such lines parallel to P′OP, $q-r=n$, where n is an integer. Hence

$$y=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$x = \sin 2\pi(t-p) - \sin 2\pi\left(t+\frac{p}{2}\right)\cos n\pi.$$

If n is even,

$$x = -\sqrt{2-2\cos 3\pi p}\,.\,\cos 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad (5)$$

If n is odd,

$$x = \sqrt{2+2\cos 3\pi p}\,.\,\sin 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad . \quad (6)$$

Equation (4) shows that along these lines the vibrations are rectilinear, and perpendicular to direction of the ray.

Putting $p=\frac{m}{3}$, m being an integer, we see from (5) and (6) that there are points of no motion when m and n are both even or both odd. These conditions will be satisfied if p, q, and r are multiples of $\frac{1}{3}$. In order to satisfy (1), one of the quantities must be an even multiple, and the other two must be both even or both odd.

We may obtain similar equations in relation to Q′OQ and R′OR; and the points of no motion will be the same as those already obtained. If we draw the three sets of lines above

considered, we shall form a series of triangles whose sides are parallel to the rays, each side being equal to $\frac{2}{3}$. These triangles will have the properties, that their angles will be nodes, and that the vibrations along their sides will be perpendicular to the sides, the displacement being given by equations (5) and (6) and the corresponding equations for the rays Q and R. The form of these triangles under displacement, when $t=0$, is shown in Pl. XII. fig. 1.

The motion may be also readily obtained along lines perpendicular to the direction of the rays, at distances $\frac{1}{3}$ from each other, one of each set passing through the origin. p must be a multiple of $\frac{1}{3}$; and there are six different forms of equations (2) and (3) for six consecutive values of p, which are given in the following Table (n being an integer):—

p.	x.	y.
$2n+1$	$(1+A)\sin 2\pi t$	$-B\cos 2\pi t$
$2n+\frac{2}{3}$	$(1-A)\sin 2\pi(t+\frac{1}{3})$	$+B\cos 2\pi(t+\frac{1}{3})$
$2n+\frac{1}{3}$	$(1+A)\sin 2\pi(t-\frac{1}{3})$	$-B\cos 2\pi(t-\frac{1}{3})$
$2n$	$(1-A)\sin 2\pi t$	$+B\cos 2\pi t$
$2n-\frac{1}{3}$	$(1+A)\sin 2\pi(t+\frac{1}{3})$	$-B\cos 2\pi(t+\frac{1}{3})$
$2n-\frac{2}{3}$	$(1-A)\sin 2\pi(t-\frac{1}{3})$	$+B\cos 2\pi(t-\frac{1}{3})$

where $\quad A=\cos\pi(q-r), \quad B=\sqrt{3}\sin\pi(q-r).$

These lines intersect the triangles (fig. 1) at their angles, and also at the bisection of their sides. At these points the motion has been already determined. The motion is circular if p is an even multiple of $\frac{1}{3}$, at the points for which $1-A=\pm B$ —that is, where $q-r=2m\pm\frac{2}{3}$ (m being an integer); and if p is an odd multiple of $\frac{1}{3}$, at the points for which $1+A=\pm B$ —that is, where $q-r=2m\pm\frac{1}{3}$.

These conditions are satisfied at the middle points of the triangles. In fig. 2 are shown the nodes and the circular points, the arrows indicating the phase when $t=0$. It will be noticed that at adjacent circular points the motion is in opposite directions.

It would be possible to construct a piece of apparatus to exhibit the motion approximately. A piece of elastic membrane, sufficiently stretched in all directions, should be fastened at a set of points corresponding to the points of rest, and the middle points of the triangles should then be displaced according to the phase (see fig. 2), and carried round their original positions in circles of equal size and period, the adjacent motions being in opposite directions—an arrangement which might easily be effected by a series of cogged wheels. We should then have a number of points fixed, and the correct motion given at other points where the motion is greatest. The motion of the rest of the membrane except near the edges would then be approximately correct.

In fig. 3 is given an enlarged view of one of the triangles, showing some of the points where the motion is elliptic, and the displacement of the lines through the nodes parallel and perpendicular to the rays.

XXXIII. *Improved Construction of the Movable-coil Galvanometer for determining Current-strength and Electromotive Force in Absolute Measure.* *By* Dr. EUGEN OBACH*.

SOME years ago I showed that the tangent-galvanometer of ordinary dimensions may be employed as a measuring instrument for very strong currents if the ring is made movable around its horizontal diameter †, a principle already adopted before that time by Prof. Trowbridge, of Harvard College, Massachusetts ‡; a little later I described a galvanometer based upon that principle, and constructed by Messrs. Siemens Brothers §.

I now propose to give a brief account of several alterations which have since been introduced by that firm, and which I venture to think render the instrument more sensitive and more convenient for use, besides creating for it a wider field.

* Read June 9, 1883.

† 'Nature,' xviii. p. 707 (1878); *Repertor. für Exper. Physik.* xiv. p. 507 (1878).

‡ Amer. Journ. of Arts and Science, vol. ii. (August 1871).

§ *Zeitschrift für angewandte Electricitätslehre*, i. p. 4 (1879).

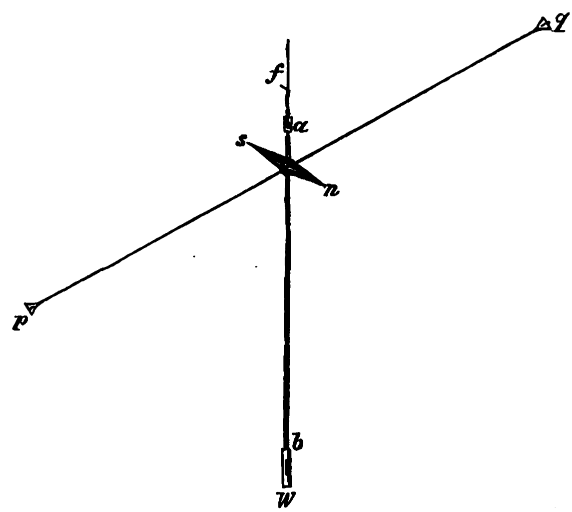
As the galvanometer, in the complete form in which I shall presently describe it, is not so much destined to meet the daily want of the practical electrician, but is rather intended for measurements where greater accuracy and trustworthiness than usual is necessary, I thought myself justified in bringing the subject before the Physical Society, particularly as the kindness of Messrs. Siemens Bros. & Co. at the same time enables me to place the instruments before you for inspection.

Ere proceeding further, allow me to say that I shall not on this occasion touch upon the theory of the instrument, which is already given elsewhere, but confine myself wholly to describing the recent improvements in its construction, adding a few series of measurements in order to prove the high degree of accuracy obtainable.

I propose to deal with the different parts of the instrument under separate headings; and will first speak of

The Magnetic Needle and its Pointer.

The older instruments had a flat magnetic needle fixed to a light vertical axle, pivotted at both ends between jewels to prevent any dipping, which the needle would otherwise expe-



Half nat. size.

rience with great inclinations of the ring. This arrangement answered sufficiently well with ordinary care; but still the

delicate pivots were likely to be damaged, thus impairing the sensitiveness of the needle. As now constructed, the dipping of the needle is completely avoided in the manner illustrated by the annexed figure. The needle, *n s*, is fixed to a thin vertical axle, *a b*, near its upper end, the lower end of the axle being provided with a cylindrical brass weight, *w*. This weight offers but little additional momentum to the whole system round the vertical axis, whereas the movement round the horizontal axis is completely prevented. The aluminium pointer, *p q*, is situated in the same plane as the scale; the ends are flattened and provided with a fine slit, which serves as an index for reading the deflections, the bottom of the needle-box being blackened. The reading can thus be taken without parallax, and therefore very accurately. The magnetic needle has a biconical shape, which entirely prevents the shifting of the magnetic axis from its original position, as was sometimes found to be the case with the old broad needles. Adjustments are provided by which the cocoon-fibre, *f*, serving to suspend the needle, can be raised or lowered, as well as accurately centred.

The Damping of the Oscillations.

Numerous experiments were undertaken to ascertain a convenient method for damping the oscillations of the needle, and to arrive, if possible, at a perfectly aperiodical movement. After trying large masses of copper placed in the immediate neighbourhood of the swinging magnet, as well as liquid damping, without decided success, air-damping was resorted to, and finally adopted. It will be remembered that Sir William Thomson used air-damping for the light-mirror of his dead-beat galvanometer, and Prof. Töpler* for other galvanometric apparatus. In our case the air-chamber consists of a shallow cylindrical box, about 8 centim. in diameter, $1\frac{1}{4}$ centim. high, provided with two radial partitions which can be slid in or out; the axle of the needle, passing through the centre of this box, carries a light and closely fitting vane. By sliding the partitions more or less into the box various degrees of damping can be obtained; and if they are right in, the motion is practically dead-beat.

* *Repert. f. exp. Phys.* ix. p. 259 (1873)

The Scales.

Declination-scale.—This scale, engraved on a horizontal ring, was formerly divided into degrees, as usually done; but now one semicircle is provided with divisions corresponding to the natural tangents. The interval between each two divisions must of course vary for different parts of the scale, and is arranged as follows:—

Table I.

Values of tangent.	Interval.
0 to 1	0·01
1 „ 2	0·02
2 „ 3	0·05
3 „ 5	0·10

It will be noticed that the value of the interval only changes at those places where the tangent is equal to a whole figure, thus making a mistake in reading less likely. Looking at this scale no gaps are conspicuous, and the divisions are everywhere pretty evenly distributed. Tangent-scales have been employed by Joule, Sir William Thomson, and others; but the one now described seems well to satisfy all the requirements.

Inclination-scale.—This scale, engraved on a vertical quadrant divided into degrees, can accurately be read to one tenth by means of a vernier. The zero division was formerly that to which the index pointed when the ring was horizontal. In this case the tangent of the deflections had to be divided by the sine of the angles. For convenience' sake, the places were specially marked on the scale at which the sines corresponded to whole figures. The new inclination-scale has the zero at the vertical or normal position of the ring; and instead of the sines, the *secants* are specially marked which are represented by whole figures. With these secants the tangents of the deflections must be multiplied; and they can therefore be termed *multiplying powers,* analogous to the multiplying power of shunts. The instruments intended only for the measurement of current-strength have the quadrant bearing the secant-scale fixed outside the ring, whilst the others, measuring also electromotive force, have it situated between the needle-box and the ring, where it is better protected from injury.

If the deflections of the needle are read on the tangent-

scale and the positions of the ring on the secant-scale, the aid of trigonometrical tables may be entirely dispensed with, as the product of the two figures represents the quantity to be measured, irrespective of a constant.

The Solid Ring and the Coil.

If the galvanometer has to serve only for the measurement of currents, the gun-metal ring is of a rectangular cross section; but if it is at the same time destined to measure difference of potential, the cross section is V-shaped, the groove being filled with numerous turns of G.S. wire. If the number of convolutions is known, and if a simple relation exists between that number and the resistance of the wire, a great advantage may be derived therefrom. For instance, if there are one thousand convolutions on the coil, offering a resistance of exactly one thousand ohms, the current due to the difference of potential of one volt at the ends of the coil would produce the same deflection of the needle as the current of one ampère flowing through the solid metal ring. That this must be so is evident, if it is remembered that the weak current of one thousandth of an ampère flows round the needle one thousand times, but the stronger current of one ampère only once. The solid ring and the convolutions are thus arranged that their cross sections have a common centre of gravity, thus both acting exactly in the same way upon the magnetic needle. If this simple plan is adopted, the calibration of the galvanometer for difference of potential in volts, which is readily performed with a few cells of known E.M.F., at the same time gives the graduation of the instrument for strength of current in ampères. I have been using various modifications of the Daniell cell with solutions of copper and zinc sulphate of equal specific gravity. At present I am engaged in constructing a standard cell for such purposes, which is always at disposal; and, as far as the preliminary experiments show, the E.M.F. of the new cell will closely approach one volt. Further on I shall communicate some measurements, by which I intend to show how accurately the calibration of the fine-wire coil in volts can serve for that of the solid ring in ampères.

The Constant Shunt and Leading Wires.

With the size of the ring usually employed, viz. 30 centim.

diameter, and our horizontal component of the earth's magnetism, currents of greater strength than about 50 ampères would require the ring to be at the multiplying powers 9 or 10, *i. e.* near the horizontal position. If, as a rule, such currents have to be measured, it is desirable to raise the constant of the galvanometer, say two or threefold, without a proportionate increase of the dimensions. This can be done by the use of a so-called "*constant shunt*," thereby allowing only half or one third of the current to flow round the needle. In our case the shunt is made of exactly the same metal as the solid ring itself; it has no soldering-places, and consists in fact of three or four little bridge pieces left standing instead of cutting the ring quite open where the terminals join. By comparison with an instrument of the same description having an open ring, the shunt can be adjusted to any power desirable.

However, by the introduction of the "constant shunt" the accuracy of the measurements is somewhat impaired. Experiments in which the shunt-pieces were touched with a thin stick of low-melting material during the passage of very strong currents, proved that they did not become hot, on account of their extremely low absolute resistance and their contact with the large metallic mass of the ring conducting away the heat. Variations of temperature, to which both the ring and the shunt are subjected, do not of course in the least disturb the ratio of their resistance, since they both consist of the very same alloy.

As the current only passes round the needle once and, if powerful enough, produces deflections even if the ring is almost horizontal, it is hardly necessary to call attention to the fact that the wires leading the current to the instrument should be so arranged that they cannot act upon the needle; still I have seen instances where this simple and almost self-evident precaution has been strangely neglected. I thought it therefore best to have special leading wires provided which are absolutely inactive upon the needle, and may therefore be named "*adynamic leads*." These leads consist of a number of well-insulated copper wires stranded together in a peculiar manner, and covered with a cotton braiding, similar to the ordinary speaking-tubes. The cable thus formed is quite

flexible, and without the slightest action upon a magnetic needle. I sent a strong current through several turns of such a cable, and held it close to a delicately suspended magnetic needle, but could not detect any effect whatever upon it. One half of the wires is covered with a differently coloured material to the other half; and the wires of each colour are united at both ends of the cable, and there soldered to a stout piece of copper wire. The adynamic cable can be made in any length and for different current-strengths; and as it offers only a small resistance, it can be employed to convey the current to be measured to a locality where the needles are not disturbed by machines or wires.

The Adjusting Parts and the Compensating Magnet.

With regard to the adjustment of the instrument, it suffices to say that, besides the necessary levelling-arrangements, it is provided with clamp-rings for tightly holding the pillar as well as the movable coil without interfering with previous adjustments, and in the same manner as is often done with mathematical apparatus. The final adjustment of the axis of the coil into the meridian is performed by means of a fine screw, which proves very useful for correcting, during a series of measurements, the occasional variations of the zero position.

As long as the galvanometer stands in the same position, the "*constant*," as a rule, changes but little even from day to day. If, however, the instrument is taken from one place to another, great changes in that respect will occur, amounting sometimes to many per cents. These changes do not of course interfere with the accuracy of the measurements, because the "constant" can easily be redetermined with a known E.M.F. at any place, as we have seen; but it would undoubtedly be very convenient to have the galvanometer of *equal sensibility* everywhere. For that purpose an auxiliary magnet is placed east or west from the needle in a plane parallel to the meridian, which can turn round a horizontal axis passing through its neutral point and the centre of the needle, and being at right angles to the diameter on which the coil is turned. This magnet does not affect the zero position, and moreover, if placed exactly vertical with its magnetic axis, it does not alter the original constant, which then only depends upon the

horizontal terrestrial component, more or less modified by the surroundings; but if it is dipped, the horizontal force acting on the needle is either augmented or diminished, according to the direction in which the magnet is turned and to the amount of dip given. It is easily seen that the magnetic influence of the surroundings upon the needle may now greatly vary from one place to another and still be compensated by the magnet, thus keeping the so-called "constant" of the galvanometer actually at a *constant value,* adjusting it, for instance, always so that the unit deflection of 45° with the vertical ring corresponds to a round number of volts and ampères, say five or ten. Under such conditions the deflection-scale could at once give the current or E.M.F. in ampères or volts. A gradual change of magnetism in the compensating magnet does not affect the measurements. I shall not further enter into this subject here, as I intend discussing it more fully on a future occasion.

Methods of Measurement.

For measuring current-strength or electromotive force either of the following four methods can be employed according to circumstances, viz.:—

I. *The General Method.*—Applicable under almost any conditions. The coil is placed in such a position that the deflection attains a proper value. If α is the deflection of the needle, and ϕ that of the coil from the meridian, the formula is

$$x = \tan \alpha \times \sec \phi \times \text{const.};$$

or in case the multiplying powers P on the quadrant are used, the formula becomes

$$x = \tan \alpha \times P \times \text{const.}$$

II. *Method of Equality.*—With this method the coil is each time placed in such a position that the needle is deflected exactly to the *same angle* ψ to which the coil is inclined, giving the formula

$$x = \tan \psi \times \sec \psi \times \text{const.}$$

Having then only to deal with a single angle for a particular measurement, these products of tangents and secants may be

calculated beforehand. For this purpose the natural sines are sufficient, because $\tan \times \sec = \frac{\sin}{\cos^2}$.

The following Table gives, for easy comparison, the values of tangents, secants, and their products at ten and multiples of ten degrees. These products, like the tangents, range from nil to infinity, but increase more rapidly.

TABLE II.

Angle.	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°
tan	0	·176	·364	·577	·839	1·192	1·732	2·747	5·671	∞
sec	1·0	1·015	1·064	1·155	1·305	1·556	2·000	2·924	5·759	∞
tan × sec	0	·1786	·3873	·6664	1·095	1·855	3·464	8·032	32·66	∞

III. *Method of Constant Deflection.*—Here the coil is each time inclined until the needle reaches a certain deflection, say $26\frac{1}{2}°$, 45°, or $63\frac{1}{2}°$, of which the corresponding tangents are $\frac{1}{2}$, 1, and 2 respectively. This figure then enters the constant, giving the simpler formula

$$x = \sec \phi \times \text{const.},$$

the instrument acting as a *secant-galvanometer*. For a given constant deflection the secant-measurements range between unity and infinity, as the above little table shows. This method has the peculiarity that the needle occupies a fixed position in space during the measurements, which in some instances may be found of advantage.

IV. *Method of Constant Inclination.*—In this case the coil is fixed at a given inclination, and sec ϕ enters the constant; thus the formula is reduced to that of the ordinary tangent-galvanometer,

$$x = \tan \alpha \times \text{const.}$$

As compared with other galvanometers proposed for a similar purpose, the one here described offers the great advantage that the magnetic needle has not to be shifted from one measurement to another, whereby the magnetic field may

sometimes considerably alter; furthermore it does not depend upon the constancy of permanent magnets, which, to say the least, is rather precarious*.

Numerical Results of Measurements.

I shall now communicate some measurements and tests to which the latest forms of instruments have been subjected, in order to illustrate the degree of accuracy obtainable. The first set was undertaken to ascertain the relation actually existing between the solid ring for currents and the coil of wire for E.M.F., which, it will be remembered, was intended to be such that ampères with the solid ring should accurately correspond to volts with the wire coil. The experiment was conducted as follows:—A current, from my constant battery with acid flow†, was sent through the solid ring and a copper voltameter in circuit for a certain time, the deflections to the right and left being observed every five minutes. The mean of these deflections was taken as corresponding to the amount of copper deposited. The copper-sulphate solution and the electrodes consisted of pure materials. The mean of the gain of the kathode and the loss of the anode was taken. The amount corresponding to one ampère per hour is 1·164 gramme of copper or 3·96 grammes of silver, the latter figure being that adopted by Messrs. Siemens and Halske, of Berlin‡. The calibration of the fine-wire coil for volts was performed by means of a number of Daniells, each compared with a Raoult's standard cell filled with pure sulphate solutions and having the E.M.F. 1·115 volt, according to Dr. Alder Wright's experiments §. The figures obtained were as follows:—

a. With the solid Ring.

Copper obtained	$=11{\cdot}435$ grammes.
Time of electrolysis	$=60$ minutes.
Mean deflection	$=47^\circ{\cdot}3$.
Position of ring P	$=$ 2.

* Another advantage undoubtedly is that the galvanometer requires no variable shunt, by which errors may very easily be introduced.

† *Rep. f. exp. Phys.* xviii. p. 633 (1882).

‡ See latest instructions for use of their torsion galvanometers.

§ Proc. Phys. Soc. v. p. 80 (1882).

Hence the current corresponding to the unit deflection of 45° with the ring vertical $=4{\cdot}531$ ampères.

b. With the fine-wire Coil.

Number of Daniells	=	4.
E.M.F. thereof	=	$4 \times 1{\cdot}109$.
Deflection obtained	=	$44^{\circ}{\cdot}6$.
Position of coil P	=	1.

Hence the E.M.F. corresponding to the deflection of 45° with the ring vertical $=$**4·525** *volts*, the correction for the resistance of the cells being applied.

These two results agree very closely indeed, showing only a difference of 0·13 per cent. This is the more remarkable as the two kinds of measurements have nothing in common, being in fact based upon data quite independent of each other, thus proving that it is admissible to substitute for such instruments the calibration in volts for that in ampères.

A further series given in Tables III. and IV., and carried out with great care, clearly show that, for any given current-strength or E.M.F., the result of the measurement is almost identical in whatever region the readings are taken.

Table III. is obtained with currents from my constant battery passed through the solid ring. The six different current-strengths were obtained by the insertion of suitable resistances, and were as nearly as possible in the proportion of the whole figures 1 to 6.

Table IV. contains measurements with the fine-wire coil, thereby using the E.M.F. of ordinary Bunsen cells connected in series, and varying in number from 2 to 12.

From these Tables it will be seen that, on the one hand, the deflections extend over the greater part of the tangent-scale, *i. e.* from $3^{\circ}{\cdot}4$ till $78^{\circ}{\cdot}2$, and, on the other, the position of the coil varies from the multiplying power 1 to 10—the quantities measurable being therefore in the proportion of 1 to 500, yet the accuracy arrived at may be pronounced fully satisfactory. Combining the results of all these measurements with the solid ring and with the wire coil, the mean error of a single reading becomes 0·35 per cent., and the probable error 0·24 per cent.

TABLE III.—Measurements with the Solid Ring.

Multiplying powers corresponding to the position of the ring.	Current-strength.																	
	C_1.			C_2.			C_3.			C_4.			C_5.			C_6.		
	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a	a.	tan a.	P×tan a
P=1	30·7	·5938	·594	49·95	1·190	1·19	·65	1·778	1·78	66·8	2·333	2·33	71·35	2·963	2·96	74·7	3·655	3·66
2	16·6	·2981	·596	30·7	·5938	1·19	41·6	·8878	1·78	49·5	1·171	2·34	56·15	1·491	2·98	61·25	1·823	3·65
3	11·2	·1980	·594	21·65	·3969	1·19	30·6	·5914	1·77	37·9	·7785	2·34	44·75	·9913	2·97	50·55	1·215	3·65
4	8·4	·1477	·591	16·65	·2991	1·20	23·85	·4421	1·77	30·4	·5867	2·35	36·7	·7454	2·98	42·5	·9163	3·67
5	6·75	·1183	·592	13·4	·2382	1·19	19·5	·3541	1·77	25·1	·4684	2·34	30·8	·5961	2·98	36·1	·7292	3·65
6	5·7	·0998	·599	11·15	·1971	1·18	16·4	·2943	1·77	21·3	·3899	2·34	26·35	·4953	2·97	31·35	·6092	3·66
7	4·8	·0840	·588	9·65	·1700	1·19	14·15	·2521	1·77	18·4	·3327	2·33	23·0	·4245	2·97	27·5	·5206	3·64
8	4·25	·0743	·594	8·4	·1477	1·18	12·5	·2217	1·77	16·3	·2924	2·34	20·35	·3709	2·97	24·5	·4557	3·65
9	3·8	·0664	·598	7·5	·1317	1·19	11·05	·1953	1·76	14·5	·2586	2·33	18·3	·3307	2·98	21·95	·4030	3·63
10	3·35	·0585	·585	6·7	·1175	1·18	10·0	·1763	1·76	13·1	·2327	2·33	16·5	·2962	2·96	20·0	·3640	3·64
Mean of P×tan a	...	...	·593	...	...	1·187	...	...	1·769	...	...	2·336	...	...	2·973	...	...	3·647
Mean error of obs.	±0·0043=·72 p. c.			±0·0060=·51 p. c.			±0·0064=·36 p. c.			±0·0069=·29 p. c.			±0·0074=·25 p. c.			±0·0101=·28 p. c.		
Prob. error of obs.	±0·0029=·49 „			±0·0040=·34 „			±0·0043=·24 „			±0·0047=·20 „			±0·0050=·17 „			±0·0068=·19 „		
Mean cur. in amp. (P×tan a×c*)	2·69			5·39			8·03			10·61			13·50			16·56		
Ratio	1			2			3			3·9			5			6·1		

* The galvanometer constant c =4·54 ampères.

TABLE IV.—Measurements with the fine-wire Coil.

Multiplying powers corresponding to the position of the coil.	Electromotive force.								
	E₁.			E₂.			E₃.		
	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan α
P=1	39·2	·8156	·816	58·2	1·613	1·61	67·4	2·402	2·40
2	22·2	·4081	·816	38·55	·8084	1·62	50·3	1·205	2·41
3	15·25	·2727	·818	28·3	·5384	1·62	38·85	·8055	2·42
4	11·55	·2044	·818	22·0	·4040	1·62	31·1	·6032	2·41
5	9·3	·1638	·819	18·0	·3249	1·63	25·8	·4834	2·42
6	7·8	·1370	·822	15·1	·2698	1·62	21·9	·4020	2·41
7	6·7	·1175	·823	13·0	·2309	1·62	19·0	·3443	2·41
8	5·8	·1016	·813	11·35	·2007	1·61	16·8	·3019	2·42
9	5·2	·0910	·819	10·1	·1781	1·60	15·0	·2679	2·41
10	4·7	·0822	·822	9·15	·1611	1·61	13·55	·2410	2·41
Mean of P×tan α	...	...	·819	...	...	1·615	...	...	2·413
Mean error of obs.	±0·0032=·39 p. c.			±0·0064=·40 p. c.			±0·0044=·18 p. c.		
Prob. error of obs.	±0·0021=·26 „			±0·0043=·27 „			±0·0029=·12 „		
Mean E.M.F. in volts. (P×tan α ×e*.)	3·72			7·34			10·95		
Ratio	1			2			2·9		

Multiplying powers corresponding to the position of the coil.	Electromotive force.								
	E₄.			E₅.			E₆.		
	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan.
P=1	72·7	3·211	3·21	76·0	4·011	4·01	78·2	4·787	4·79
2	58·05	1·603	3·21	63·5	2·006	4·01	67·4	2·402	4·80
3	46·9	1·069	3·21	53·2	1·337	4·01	58·1	1·607	4·82
4	38·7	·8012	3·21	45·15	1·005	4·03	50·2	1·200	4·80
5	32·7	·6420	3·21	38·7	·8012	4·01	43·8	·9590	4·80
6	28·0	·5317	3·19	33·65	·6657	3·99	38·6	·7983	4·79
7	24·5	·4557	3·19	29·75	·5716	4·01	34·45	·6860	4·80
8	21·8	·4000	3·20	26·5	·4986	3·99	30·9	·5985	4·79
9	19·55	·3551	3·20	24·0	·4452	4·01	28·15	·5351	4·82
10	17·7	·3191	3·19	21·7	·3979	3·98	25·55	·4781	4·78
Mean of P×tan α	...	...	3·201	...	...	4·003	...	...	4·798
Mean error of obs.	±0·0083=·26 p. c.			±0·0119=·30 p. c.			±0·0129=·27 p. c.		
Prob. error of obs.	±0·0056=·17 „			±0·0084=·21 „			±0·0087=·18 „		
Mean E.M.F. in volts. (P×tan α ×e*.)	14·53			18·17			21·78		
Ratio	4			5			6		

* The galvanometer constant e =4·54 volts.

TABLE V.

Position of compensating magnet.	Extra resistance inserted.	Sine of inclination.						Mean of $\frac{\tan \alpha}{\sin \phi}$.	Mean error of observation.
		$\sin \phi$	·2	·4	·6	·8	1·0		
Dipped to augment earth's magnetism.	0	deflect. α	22°·1	38°·9	50°·4	58°·2	63°·5	2·020	±0·005 or 0·25 p. c.
		$\tan \alpha$	·4061	·8069	1·209	1·613	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·03	2·02	2·02	2·02	2·01		
Vertical, having no action.	0·13 ohm.	deflect. α	21°·9	38°·7	50°·3	58°·0	63°·5	2·006	±0·004 or 0·20 p. c.
		$\tan \alpha$	·4020	·8012	1·205	1·600	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·01	2·00	2·01	2·00	2·01		
Dipped to reduce earth's magnetism.	0·56 ohm.	deflect. α	21°·9	38°·9	50°·4	58°·2	63°·5	2·016	±0·004 or 0·20 p. c.
		$\tan \alpha$	·4020	·8069	1·209	1·613	2·006		
		$\frac{\tan \alpha}{\sin \phi}$	2·01	2·02	2·02	2·02	2·01		

Fig. 1.

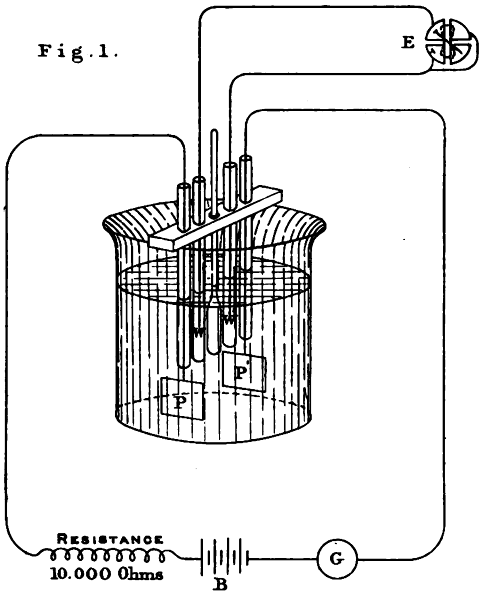
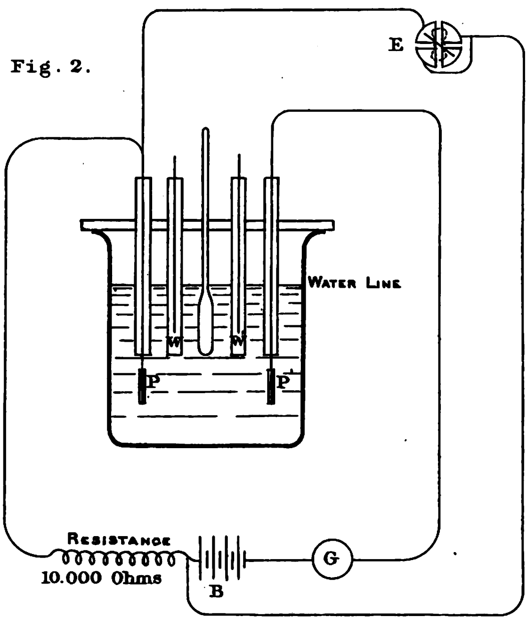


Fig. 2.



Mintern Bros. lith.

The last set of measurements had for its object to show that a compensating-magnet of the description proposed does not affect the readings. The results are embodied in Table V.; they were obtained with one of the older forms of solid-ring galvanometers provided with a sine-scale. The curved controlling magnet of a mirror-galvanometer, 20 centim. long and 2 centim. broad, was strongly magnetized and placed a a distance of 24 centim. in the manner formerly specified, and so arranged that it could be turned on a horizontal axis. Three different positions were given to the magnet—viz. one, in which it assisted the earth's magnetism, another, in which it did not act upon the needle, and a third, in which the earth's magnetism was partly neutralized. By altering the resistance in circuit, the deflections with the vertical ring were made equal in all three cases, viz. $63^{\circ}\cdot 5$.

Table V. shows that the degree of accuracy did not materially differ under the three varying conditions. The magnet therefore does not appreciably interfere with the measurements. The mean error of all three positions of the magnet is 0·22 per cent., which is very low.

In conclusion, I may mention that a smaller model of the galvanometer, intended for practical use, is now being made, which will contain all the recent improvements, viz. the fine-wire coil besides the solid ring, the tangent-scale, the secant-marks, the air-damping, and the compensating-magnet. The latter will be so arranged that the "constant" will be considerably increased as compared with that due to the earth's magnetism alone; thus the needle should be much less influenced by outer disturbances than before.

Woolwich, June 1883.

XXXIV. *Note on the Measurement of the Electric Resistance of Liquids. By* Professors W. E. Ayrton, *F.R.S., and* John Perry, *M.E.**

[Plate XIII.]

Some time back a paper was communicated by Professors Reinold and Rücker to this Society on the Resistance of Liquid Films, which had a double interest, arising from the great

* Read June 9, 1883.

value of the results arrived at and from the method employed to obtain them. It is of course well known that the great difficulty in measuring the resistance of a liquid arises from the polarization of, or actual deposit of gases on, the anode and cathode, which makes the apparent resistance of the liquid far greater than the true value. To overcome this difficulty Kohlrausch employed rapidly alternating currents; and Dr. Guthrie, with Mr. Boys, dispensed altogether with the anode and cathode by observing the amount of twist produced in a fine steel wire supporting a vessel of liquid when a magnet was rotated at a fixed speed in the neighbourhood.

But there is another method of measuring the resistance of a liquid independently of its polarization—the one so successfully employed by Profs. Reinold and Rücker, and which consists in measuring by means of an electrometer the potential-difference at two fixed points in a column of the liquid when a current of known strength is passing through it.

At the time Profs. Reinold and Rücker communicated their paper, we mentioned that some years previously certain experiments had been conducted in our laboratory in Japan for the purpose of ascertaining how far the electrometer method of measuring the resistance of a liquid was entirely independent of polarization; and as we have since come across the results of these experiments in turning over some papers, we have thought that the information may possess some interest for the Members of this Society. The experiments were made at the commencement of 1878 by some of our students; and the first part of the investigation was for the purpose of ascertaining how the resistance of water varied with the electromotive force employed and with the temperature of the water when, first, the resistance was measured by the current which a known electromotive force could send between platinum plates of known size and at fixed distances apart in the water, and, secondly, when the resistance was measured by a comparison of the potential-differences of two platinum wires placed in the water at fixed distances apart, with the potential-differences when the same current was being sent through a known resistance.

Figs. 1 and 2, Pl. XIII., show the arrangement of the apparatus used in the experiments. B is the battery producing the

current passing between the platinum plates P and P′. G is a delicate reflecting galvanometer measuring the current. E is a quadrant-electrometer which measures the difference of potentials between the two wires W and W′. These two platinum wires W and W′ were immersed in glass tubes; and their ends were above the bottom of the glass tubes as shown. Figure 1 shows the connexions when the differences of potentials between W and W′ were being measured by the electrometer, and figure 2 when the differences of potentials at the two ends of the known resistance-coil, of 10,000 ohms, were being measured.

The following Table gives the dimensions of the various parts of the apparatus:—

Diameter of the beaker at water-line	8·5	centim.
Height of water-line above the bottom ...	5·76	„
Distance between centres of wire tubes (W, W′ in fig. 2)	4·88	„
Distance between the platinum plates......	7·3	
Part of the glass tube surrounding the wire dipped in water.....................	2·14	„
Part of the platinum wire in water.........	0·91	„
Outside diameter of the glass tube	0·87	„
Size of platinum plate : height	3·28	„
„ „ „ : width	2·29	„

Before each experiment, when no current was passing, the difference of potentials between the plates and wires was reduced to 0, if not 0 already. The wires W and W′ were heated to redness before each experiment, and the platinum plates cleaned.

At the beginning pure distilled water was used; and this water was not added to all the time: it therefore lost a little by evaporation during the course of the experiment, and may have become a little dusty; but as the main object of the investigation was to examine the method of testing, and not for the purpose of measuring the specific resistance of water or of any other particular liquid, this result was of little consequence.

The following is a sample of the experiments made:—

January 25, 1878.—Battery-power employed $\frac{1}{6}$ of 23 Daniell's cells, having an E.M.F. of 4·08 volts, and which gave a deflection of 468 divisions on the galvanometer when

shunted with the $\frac{1}{749}$ shunt, and when a resistance of 10,000 ohms was introduced in the circuit.

Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.		Temperature.
		Figure 1.	Figure 2.	
1m	99	10		59°·5 F.
2	96	10		
3	94	10		
Plates and wires thoroughly discharged.				
1m	99	......	14	61° F.
2	98	......	14	
3	90	......	14	

Time after putting on battery.	Resistance as determined by the galvanometer.	Resistance as determined by the electrometer.
1m	37000	7100
2	38000	7100
3	39000	7100

The annexed Table gives the results of a long series of experiments:—

Total electromotive force, in volts.	Temperature.	Resistance as determined by galvanometer at end of one minute.	Ratio of resistance at the end of second minute to the resistance at the end of the first.	Ratio of resistance at the end of third minute to the resistance at the end of the first.	Resistance as determined by electrometer.	Ratio of resistance determined by galvanometer to resistance as determined by electrometer.	Date.
0·93	58° F.	93900	1·27	1·4	15000	6·26	23rd Jan.
	·60	133000	1·12	1·37	15000	8·87	23rd „
A 0·93	100	56000	1·16	1·27	8000	7·00	24th „
1·86	63	53000	1·10	1·14	10670	5	24th „
1·86	62	51500	1·12	1·18	10300	5	24th „
B 1·86	102	32000	1·17	1·27	9670	3·31	24th „
4·08	59·5	37000	1·03	1·05	7100	5·21	25th „
4·08	102	19800	1·01	1·06	3270	6·06	28th „
6·17	60	21000	1·0	1·0	4450	4·72	28th „
C 6·17	106	12000	1·08	1·08	2700	4·44	31st „
16·45	62	12700	1·01	1·99	3170	4·01	31st „
D 16·45	107	7700	1·04	1·04	1953	3·94	31st „

Only one number is given for the resistance as determined by the electrometer in each case, because it was found not to vary much during the time of electrification; whereas the resistance as determined by the galvanometer, as will be seen, increased in the earlier experiments 30 to 40 per cent. during the three minutes' electrification.

The total electromotive force in each case was determined by making a comparison by means of the electrometer with one of Clark's standard cells.

From these observations the following conclusions may be drawn:—First, the resistance measured by the galvanometer is much greater when using about 1 volt than when using nearly 2, at the same temperature (compare observations A and B), whereas the electrometer-measurements altered very little at all. Again, comparing C and D, we see that the resistance measured by the galvanometer is much greater when using 6 volts than when using 16. In this case, however, the measurements of the electrometer are also considerably greater in the first case than in the second, the temperature being the same. Secondly, if the electromotive force is less than the decomposing electromotive force, then the smaller it is the more does the resistance alter from one to two minutes' electrification, and from two to three minutes'. Whenever, however, the electromotive force is sufficiently high for decomposition to take place, the electrification seems to produce but little change in the resistance. The resistance of the water diminishes as the temperature rises, the electromotive force being kept constant.

The following experiments were made preliminarily to explorations of the region between the two platinum plates in the water, for determining what were the directions of the lines of flow of current. We desired to see if there was any chance of being able to use platinum wires in glass tubes connected with the electrometer, as previously described.

In the following cases a long trough of water was used instead of the beaker.

The sensibility of the galvanometer was nearly the same throughout all the experiments, and was such that $\frac{1}{20}$ of the whole electromotive force employed produced a deflection of about 500 divisions when there was an external resistance

of 10,000 ohms and when the multiplying-power of the shunt employed was 100·7, which shunt was used throughout all the experiments.

Four Menotti cells, having an electromotive force of 3·7 volts, were employed in each of the following experiments. In A, B, C, D, E, F, and G the two platinum plates were placed parallel to one another at a distance of 90 centimetres apart. The two wires and their glass tubes were placed to commence at a distance of 80 centim.—that is, each being 5 centim. from the platinum plate. The lower ends of the

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature of water.
February 21, 1878.					
	centim.	m s			
A	80	1 0	669	53	13° C.
	60	1 30	662	40	
	40	1 50	658	27	
	20	2 15	654	15	
B	80	1 0	667	52	13° C.
	60	1 25	663	39	
	40	1 45	657	27	
	20	2 10	653	15	
February 22, 1878.					
C	80	1 0	680	50	13° C.
	60	1 35	675	37	
	40	2 10	672	26	
	20	2 30	670	15	
D	80	1 0	685	49	13° C.
	60	1 20	680	37	
	40	1 50	677	24	
	20	2 10	674	12	
E	80	1 0	697	48	13° C.
	60	1 20	692	36	
	40	1 50	688	24	
	20	2 20	685	13	
F	80	1 0	699	50	13° C.
	60	1 35	694	37	
	40	1 50	690	25	
	20	2 15	687	13	
G	80	1 0	over 717	51	13° C.
	60	0 30	"	38	
	40	2 0	"	25	
	20	2 20	"	15	

platinum wires were each $1\frac{1}{2}$ centim. above the lower ends of the glass tubes; and the lower ends of the glass tubes were 1 centim. below the surface of the water. The two platinum plates and one of the platinum wires were kept immovable, while the other platinum wire was moved along the trough.

The object, of course, of taking the galvanometer-readings was to ensure that no material change was taking place in the current through the weakening of the battery or otherwise while the experiment was being made.

The experiments E and F appear most satisfactory of this set; and from these it seems that the resistance of the upper layer of water-column is nearly proportional to the distance between the platinum wires, except for the nearest distance, n which case the column seems to have a slightly larger resistance than it ought to have. This perhaps arose from the fact that, although the platinum plates nearly filled up the entire section of the trough, still the lines of flow at the platinum wire, which was kept stationary at a distance of 5 centimetres from one of the plates, were not quite parallel to the edge of the trough.

The two following sets of experiments, H and I, differ from the preceding only in that the lower end of the glass tube was one centimetre above the bottom of the trough; and from these two sets of experiments we see that the resistance of the lower layer of water-column, as measured by the electrometer, is nearly proportional to the distance between the wires, except, again, for the shortest distance.

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.
		m s		
H	80	1 0	709	50
	60	1 35	703	37·5
	40	2 0	700	26
	20	2 20	698	13
I	80	1 0	704	49
	60	1 20	700	37
	40	1 45	796	25
	20	2 20	792	13

The next experiments were for the purpose of seeing whether

the potential, as measured by the electrometer, would come out uniform at all points in one vertical transverse section of the trough as well as at all points in one of the glass tubes.

Distance between the platinum wires W and W′, in centimetres.	Position of the lower end of one of the tubes.	Galvanometer-deflection.	Electrometer-deflection.
J 80	Up.	696	49
J 80	Down.	692	49
J 80	Up.	689	49

"Up" means that the lower end of the glass tube was about 1 centim. below the surface of the water; and "Down" that it was about 1 centim. above the bottom of the glass trough. The platinum wire was now raised about 4 centim. above the bottom of the glass tube when the glass tube was down and the electromotive force was unaltered. The potential therefore, at all points in a vertical transverse section as well as at all points in the glass tube, is the same as measured by the electrometer.

The next set of experiments, K and L, were made under exactly the same conditions as A, B, C, D, E, F, and G, with the exception that the terminal platinum plates were now perpendicular to each other, the plate towards which the wire was moved being parallel to the long side of the trough.

Distance between the platinum wires, in centimetres.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature.
	m s			
K 80	1 0	706	51	13° C.
K 60	0 30	701	38	13° C.
K 40	0 50	699	27	13° C.
K 20	2 0	697	15	13° C.
L 80	1 0	705	51	13° C.
L 60	0 30	701	40	13° C.
L 40	0 55	698	27	13° C.
L 20	2 20	697	15	13° C.

The resistance of the longer column of the water as measured by the electrometer is about the same as before, whereas that of the shorter is even greater; so that the resistance for the 80-centimetre column is even still less than four times that for the 20-centimetre one. But since the platinum plate near the stationary platinum wire was in these last two sets of experiments K and L kept parallel to the trough (that is, parallel to the mean direction of the lines of flow), it follows that any want of parallelism of the lines of flow to the edge of the trough at the point where was the stationary wire would be exaggerated by this mode of placing the plate; and since we observe that the error in the proportional law for distance is also increased, we may conclude that the explanation given above of the want of perfect accuracy in the proportional law being due to want of perfect parallelism in the lines of flow is the correct one.

In all the previous experiments the distance between the electrometer-wires only was altered; but in the next set the distance between the platinum plates as well as that between the platinum wires was altered, the distance between each plate and wire being kept constant. Further, the resistance determined from the electrometer was calculated, not, as before, by comparing the electrometer-deflection when its electrodes were attached to the platinum wires with the deflection obtained when its ends were attached to a known resistance traversed by the same current, but by first determining the absolute value, in volts, of the electrometer-scale with the absolute value, in ampères, of the galvanometer-scale, and by observing the electrometer- and galvanometer-deflections in each experiment.

Battery-power employed, 4 Menotti's cells. Temp. 14° C.

Distance between the platinum plates 20 centim. „ „ „ wires 10 „				
			Shunt $\frac{1}{990\cdot2}$.	
Time after putting on battery, in minutes.	Galvanometer-deflection.	Electrometer-deflection.	Resistance, as determined by galvanometer, in ohms.	Resistance, as determined by electrometer, in ohms.
1	230	19	27000	11340
2	220	19	28000	11760
3	214	18·5	29000	11900
4	210	18	29600	11800
5	206	17	30000	11400
Distance between the platinum plates 90 centim. „ „ „ wires 80 „				
			Shunt $\frac{1}{100\cdot7}$.	
1	634	42	97000	90200
2	621	42	98980	92070
3	614	42	99960	92900
4	608	41	100940	91900
5	602	41	111960	101900
Distance between the platinum plates 90 centim. „ „ „ wires 80 „				
			Shunt $\frac{1}{100\cdot7}$.	
1	633	43	97000	93100
3	621	42	98000	91100
4	614	42	99000	92100
5	609	42	100000	93000
6	604	41	101000	92000
Distance between the platinum plates 20 centim. „ „ „ wires 10 „				
			Shunt $\frac{1}{990\cdot2}$.	
1	245	21	25500	11900
2	235	20	26600	11700
3	230	20	27200	11960
4	225	19	27800	11670
5	220	19	28400	11930

The resistance, therefore, as measured by the galvanometer,

does not increase as rapidly as the distance separating the plates, while that as measured by the electrometer is fairly in proportion to the distance. The explanation of the former is probably due to the fact that, since the electromotive force employed in all these four sets of experiments was constant, a greater current flowed when the plates were nearer than when they were far apart, hence that the resistance due to the layer of gas was greater when the plates were near than when they were far.

And this leads to a simple method of accurately measuring the resistance of liquids by using a galvanometer. The method, which was independently arrived at by one of our assistants (Mr. Mather), is now employed in our laboratory, and is so simple that we feel it can hardly be novel. It is as follows :—In a long vertical glass tube containing the liquid there are two metallic disks, not necessarily of platinum, and of about the same diameter as the tube. One of these can slide up and down the tube, so as to be able to be set at any fixed distance from the other. The disks are first put tolerably far apart, and a certain convenient current made to flow, which is measured on a galvanometer in the circuit. The plates are now made to approach and the current kept exactly the same by the insertion of an external resistance; whence it follows that the resistance of the column of liquid which has been subtracted from that originally separating the plates is equal exactly to the external resistance necessary to be inserted to keep the current constant.

February 28, 1878.

The next set of experiments was made to determine the alteration in resistance of a long trough of water when the distance between the centres of the platinum plates was kept constant at 90 centimetres, and the positions of the platinum plates varied as shown in the figures.

Galvanometer-Constant.—4 Menotti's cells with an E.M.F. 3·8 volts gave a deflection of 618 when a resistance of 10,000 ohms was in circuit and the galvanometer shunted with the $\frac{1}{990\cdot 2}$ shunt.

The 4 Menotti's cells were employed and the galvanometer shunted with the $\frac{1}{100\cdot 7}$ shunt, and the readings were

in each case taken one minute after the application of the battery.

	Position of plates.	Galvanometer-deflection.	Temperature.
I.	— —	642	16° C.
	╲ ╲	640	
	│ │	622	
II.	│ │	634	16° C.
	╲ ╲	643	
	— —	647	

Two sets of experiments in the reverse order were taken to eliminate any change that might take place in the deflection from weakening of the battery, or from polarization of the plates, or from set of the galvanometer-fibre. The constant distance between the centres of the plates was now diminished to 20 centimetres, when the following results were obtained, the $\frac{1}{990{\cdot}2}$ shunt being employed.

	Position of plates.	Galvanometer-deflection.	Temperature.
III.	│ │	245	16° C.
	╲ ╲	291	
	— —	304	
IV.	— —	315	16° C.
	╲ ╲	285	
	│ │	239	

Both therefore at the greater and at the less distance the resistance is least when the platinum plates are edge on; a result that could hardly have been expected for the longer distance, considering that the width of each plate was only about 6 centimetres.

March 1, 1878.

In the following experiments one plate only was turned. The galvanometer had about the same sensibility as before. The $\frac{1}{100{\cdot}7}$ shunt was used when the distance between the

centre of the plates was 90 centimetres, and the $\frac{1}{990\cdot2}$ shunt when it was 20 centimetres. An electromotive force of 3·8 volts was employed in each test.

Distance between the centres of the plates 90 centimetres.

	Position of plates.	Galvanometer-deflection.	Temperature.
V.	| |	633	15° C.
	| \	634	
	| —	637	
VI.	| —	660	15° C.
	| \	655	
	| |	651	

Distance between the centres of the plates 20 centimetres.

	Position of plates.	Galvanometer-deflection.	Temperature.
VII.	| —	403	15° C.
	| \	368	
	| |	358	

Battery reversed.

	Position of plates.	Galvanometer-deflection.
VIII.	| |	208
	| \	211
	| |	229

Here again, then, the resistance is least with the plate end on, even when the distance between the centres of the plates is as much as 80 centimetres.

This apparent anomaly of the smaller resistance obtained when one or both plates is put end on is, as was pointed out by Mr. Boys, probably due to the smaller density of the gas which is deposited on a plate when it is put end on (in consequence of the current flowing from both sides of the plate into the liquid under these circumstances) more than compensating for the want of parallelism of the lines of flow when one or both of the plates are put end on.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

OCTOBER 1883.

XXXV. *Description of an Apparatus to illustrate the Production of Work by Diffusion.* *By* C. J. WOODWARD, *B.Sc.**

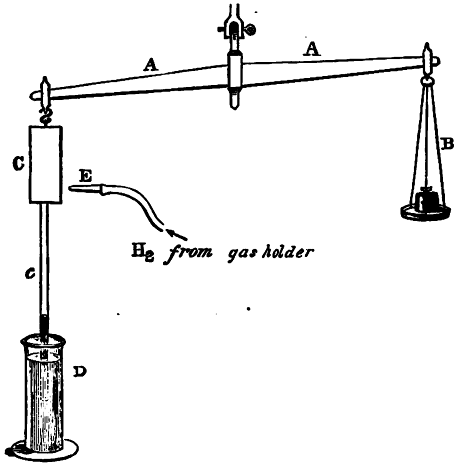
DIFFUSION, as a source of energy, is usually shown in the lecture-room by bringing a jar of hydrogen over a porous vessel fitted up with a glass tube dipping into water. Hydrogen, by inward diffusion, enters the jar; the internal pressure thus produced forces the water down, and a stream of bubbles escapes from the tube. On removing now the jar of hydrogen, outward diffusion of the hydrogen takes place, a minus pressure is produced in the porous vessel, and the water is lifted.

The apparatus I am about to describe is an adaptation of this experiment to the production of an oscillatory movement of a beam from alternate inward and outward diffusion of hydrogen.

The apparatus is represented in the annexed figure. A A is a scale-beam about 3 feet long, carrying at one end a scale-pan and counterpoise, B, and at the other the porous jar, C, fitted with a cork, and a glass tube *c* which dips into a gas-jar, D, containing water or methylated spirit. Three or four glass jets, of which one is shown at E, are supported in a horizontal position, and the opening of each jet is placed as near as possible to the porous vessel without touching it during the oscil-

* Read May 12, 1883.

lations of the beam. These jets are connected by means of a flexible tube with a gas-holder containing hydrogen; the bell of



the holder being loaded so as to force the hydrogen in a gentle stream against the sides of the porous vessel. The best position for the jets is found by trial; but usually I place them a little below the middle of the porous jar when the beam is horizontal. The action of the apparatus is simple. On turning on the hydrogen, inward diffusion takes place, producing plus pressure within the jar; this pressure is resisted equally in all directions but the vertical, and in this direction, owing to the little friction of the water, movement takes place, and the jar rises. When the jar has risen above the jets, inward diffusion diminishes, or perhaps ceases, while outward diffusion of the hydrogen commences; a minus pressure is thus produced in the porous vessel, and the external pressure of the air causes it to descend. This descent brings the jar again opposite the jets, when the series of movements again begins.

The work done with this arrangement is very small, and falls far short of the theoretical value*. For the best effect

* "The work that may be done during the mixing of the volumes v_1 and v_2 of two different gases is the same as that which would be gained during the expansion of the first gas from volume v_1 to volume v_1+v_2, together with the work gained during the expansion of the second gas from v_2 to v_1+v_2, the expansion being supposed to be made into vacuum." See a paper by Lord Rayleigh in the Phil. Mag. [4] xlix. p. 311.

the jar should be surrounded by hydrogen for inward diffusion to take place, and subsequently the connexion with the hydrogen should be completely cut off and air take its place. I have tried to devise some water-seal arrangement by which the flow of hydrogen could be turned off and on at the right moments by the movement of the beam; but have not succeeded, as the friction thus introduced would be more than the movement of the beam could overcome. In the arrangement exhibited there is a considerable waste of energy due to the imperfect cut-off of the hydrogen, even when the flow of gas has been regulated so as to obtain the maximum effect.

XXXVI. *Experiments on the Velocity of Sound in Air.* *By* D. J. Blaikley*.

The method of experiment which I venture to bring before you this afternoon is the outcome of various attempts made by me to determine with greater accuracy than had hitherto been done the velocity of sound in small tubes, such as are used for musical instruments; in addition to which practical purpose the idea presented itself to my mind that, if a series of tubes were taken, having their diameters in a definite ratio, the observed results might by calculation be extended so as to include a value for a tube of infinite diameter, that is for free air. Some of the results were brought before the Musical Association in June last; but the experiments have since been repeated with greater accuracy.

It will not be necessary to take up your time by referring to more than a few of the many determinations that have been made. A useful summary was given by Mr. Bosanquet in the 'Philosophical Magazine' for April 1877; but we may note that only those observations in which corrections both for temperature and for moisture have been made can be considered at all accurate.

Such corrections being made, there remained at the time of Regnault's great series of experiments† considerable differences in the results arrived at by different observers, partly due,

* Read November 10, 1883.

† *Mémoires de l'Académie des Sciences*, tome xxxvii.

doubtless, to errors of observation and partly to an assumption of the absolute correctness of Laplace's formula, the theory in the application of which is that the excess of pressure in the wave above the barometric pressure of the air is infinitely small.

Experiments on the velocity of sound may be classed as open-air experiments and laboratory experiments; and I venture to think that the latter offer advantages which cannot be enjoyed in open-air work. The usual method in the open air has been for an observer at a distance from a gun to note the time which elapses between the flash and the hearing of the report; but even when the actual record of the time is aided by electrical or other apparatus, some difficulties and sources of error remain. For instance, the accurate registration of temperature and moisture is difficult, especially when the sound-wave passes at various heights above the earth's surface, as is the case when the experiment is carried out on two hills separated by a valley.

Many of these gun-fire determinations were critically examined by Le Roux*, and an estimated correction made for errors in temperature, the readings having been in all probability too high for air some metres above the ground.

Midway in character between open-air and laboratory experiments stand those of Regnault, carried out in gas- and water-mains; one of his reasons for choosing this method being the facility afforded by these tubes for the accurate observation of temperature and moisture. Passing by his work for the moment, we may note the laboratory method employed by Kundt, and also Le Roux's method, the latter giving 330·7 metres at 0° C. Kundt's method consists in its best form in the use of two glass tubes connected by a smaller tube or rod of glass, wood, or metal, this connecting-rod being clamped in certain positions to establish nodes, and its free vibrating ends being fitted with pistons working in the large tubes. The waves are excited by friction in the vibrating rod and transmitted therefrom to the air or other gas in the tubes, and the successive half-wave lengths are registered by the positions assumed by lycopodium dust during the vibration. By using tubes of different diameters he obtained the results

* *Ann. de Chimie*, ser. 4, tom. xii. Nov. 1867.

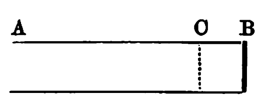
shown in Table I., and came to the conclusion that the velocity observed in his largest tube was not appreciably different from the velocity in free air. He appears, however, to have experienced some difficulty in the determination of pitch, owing to the evanescent character of the sound. The intensity also was not registered. The method is beautifully adapted for comparative rather than for absolute results.

TABLE I.

Velocity of Sound in Tubes, in metres per second, at 0° C.
(Kundt.)

Diameter of tube. millim.	Velocity. metre.	Difference. metre.
55	332·80	
		·07
26	332·73	
		2·26
13	329·47	
		6·47
6·5	323·00	
		7·58
3·5	305·42	

Le Roux's method consisted in employing a U-shaped tube, 0·07 metre (70 millim.) diameter, closed by a membrane at each end. One membrane was tapped with a small beater, and the time occupied by the resulting wave in travelling between the two membranes, as indicated by the disturbance of the second one, was registered. It appears to me, however, that the employment of membranes may introduce a source of error in this way:—Let A B be a tube closed by a rigid material at one end, and of a length to give the maximum resonance to a quarter-wave. Now, instead of the rigid end, let the tube be closed by a membrane: this will require to be in the position C, *i. e.* nearer to A than B is, the exact position depending upon the tension of the membrane. In Le Roux's experiments, unless the two membranes were of exactly the same tension, a source of error would be introduced.



We may now turn to Regnault's experiments, a summary of which is here given.

TABLE II.

Velocity of Sound in Tubes, in metres per second, at 0° C. (Regnault.)

Diameter of tube. millim.	Velocity (mean). metres.	Velocity (limiting). metres.
1100	330·5	330·3
300	330·3	329·25
108	327·52	324·25
Free air.	330·71	330·60

The one point which appears to me to be open to question is whether the rate of diminution of velocity is so great as his work appears to prove; for if this rate of diminution is extended until we reach tubes of the size used in musical instruments, we should have a velocity much less than experiments show to exist in such tubes (compare with Kundt). Probably the want of perfect smoothness in Regnault's tubes may account for some of the difference; but I think that it is doubtful whether the influence of the membranes closing his tubes was thoroughly allowed for, and feel that the question is still an open one.

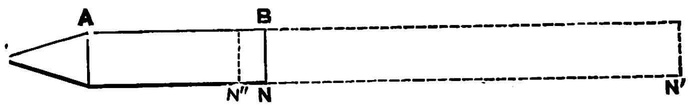
It has long appeared to me that useful results might be obtained by making use of tubes giving musical notes, as the pitch of a steadily sounding note can be readily determined within a remarkable degree of accuracy, and there should be no difficulty in determining the temperature within half a degree Fahr., which is equivalent to 6 inches in the velocity. (Experiment—Resonance of closed tube to fork of 512 vibrations, the length of the tube being modified during the experiment to show both the maximum and constrained or imperfect resonance.) (See paper in Phil. Mag. for May 1879.)

Modifying this experiment by sounding the tube with an organ-pipe mouth, the disturbing influence of the contraction of area at the lip comes into play in addition to the correction for the open end; so that, although the value of the latter is pretty accurately known, no measurement of velocity based merely upon the length of a pipe is at all reliable. By adding, however, a half-wave or wave-length to a stopped pipe, maintaining the original pitch, and measuring this added length, I hoped to be able to get reliable results. The observations,

though agreeing very well indeed so long as the pressure remained constant, did not agree in their different series when taken under very slight differences of pressure, much less than would cause sensible variation through change of intensity, according to Regnault's determinations.

To properly understand the causes of these variations, it will be necessary to examine some of the results of imperfect or constrained resonance, and consider the separate and conjoined influences of (1) the blast, air-reed, or inducing current; (2) the prime tone of the resonating air-column; and (3) the higher or partial tones sounding with the prime; the pitch of the resultant note, or alternating induced current, depending upon the values of these three forces, which have a power of mutual influence within certain limits. The subject presents itself to my mind in the following way:—

In the figure, let the blast from A cause the quarter-wave length of pipe AB to speak a note of a certain pitch, say 105 vibrations, and let BC be the half wave-length of pipe added to AB; the three-quarter wave-length AC being tuned to



speak the same note as AB. Assuming values in four typical cases, we obtain the following results (p. 324), in working out which arithmetical means instead of mean proportionals have been taken for the sake of numerical simplicity, although perhaps the latter would be more correct.

In Cases 1 and 2 the length NN′ is the true length of a half-wave of 105 vibrations. In (1) the position of N (the node) would remain unchanged on the addition of the half-wave; but in (2) the three-quarter wave would be of $106\frac{2}{3}$, and N would change its position to N″: this, however, would introduce no error in the result, for $3(106\frac{2}{3})-110=2\times105$, the length for half-wave as determined by the positions of N and N′.

In these two cases it is assumed that the first quarter-wave has equal constraining-power with the added quarters; but this is not strictly correct for cylindrical organ-pipes, in which the first quarter-wave is shortened and the mass in vibration

	Unconstrained Pitch.	Difference.	Constraining power or stability.	Difference × Power.	Values of Components.	No. of Components.	Pitch of note heard.
CASE 1.—Blast and air-column of same pitch.							
Blast from A	105	....	1	....	105	÷ 2	= 105
Quarter-wave, AB	105	....	1	....	105		
					210	÷ 4	= 105
Add half-wave, BC	105	....	1	....	210		
CASE 2.—Blast and air-column of different pitches.							
Blast	100	....	1	....	100	÷ 2	= 105
Quarter-wave	110	10	× 1	= 10	110		
					210	÷ 4	= 105
(a) Add half-wave, BC	105	5	× 1	= 5	210		
Or, Blast	100	....	1	....	100	÷ 4	= 105
(b) Three-quarter-wave	$106\frac{2}{3}$	$6\frac{2}{3}$	× 1	= $6\frac{2}{3}$	320		
CASE 3.—Blast and air-column of different pitches.							
Blast	100	....	1	....	100	÷ 2	= 105
Quarter-wave	115	15	× $\frac{2}{3}$	= 10	110		
With added half-wave.							
Blast	100	....	1	....	100	÷ 4	= 105
Quarter-wave	$107\frac{1}{2}$	$7\frac{1}{2}$	× $\frac{2}{3}$	= 5	105		
Half-wave	$107\frac{1}{2}$	$7\frac{1}{2}$	× 1	= $7\frac{1}{2}$	215		
CASE 4.—(Case 2 modified by presence of 12th of prime or 3rd partial.)							
(a) Blast	100	....	1	....	100	÷ 3	= 105
Quarter-wave (prime)	105	....	1	....	105		
„ „ (3rd partial)	$\frac{330}{3}$	....	1	....	110		
(b) Blast	100	....	1	....	100	÷ 3	= $103\frac{1}{3}$
Quarter-wave (prime)	105	....	1	....	105		
„ „ (3rd partial)	$\frac{330}{3}$	10	× $\frac{1}{2}$	= 5	105		

reduced, through the contraction of the opening at the speaking end. The tendency of this would be to influence the results as shown by Case 3, where the first quarter-wave is assumed to have a mass of $\frac{2}{3}$ instead of unity, and in which the measured length would be represented by $3(107\frac{1}{2})-115 = 207\frac{1}{2}$, whereas it should be $2\times105=210$.

In Case 4 (a), suppose the point N to have been determined for the pitch as heard, 105, with a quarter wave-length of tube. A half-wave is added, and tuned to the original pitch, $=105$, and NN′ is taken as its measure of length. This is correct if the relative strength of the partials remains the same; but if a modification such as (b) takes place, the third partial falling to half-strength, the position of N as observed would no longer be correct for a pitch of 105 but only for a pitch of $103\frac{1}{3}$. A further complication would arise in this case, through the introduction of the point considered in Case 3.

To eliminate these sources of error, one of two things must be attained: we must either deal with pure tones or be careful to have resonating pipes of such form as to have their various proper tones strictly in accord with the harmonic series, and not merely approximately as is the case with organ-pipes; and in either case be careful that the blast is of exactly the power to give the desired note without constraint.

The tubes with which I experimented were of four diameters—·45, ·75, 1·25, and 2·08 inches respectively (11·7, 19·5, 32·5, and 54·1 millim.). After detecting the possible sources of error which have just been described, and trying a few modifications of the ordinary organ-pipe, the speaking ends or first quarter-wave lengths of the tubes were modified in form from cylindrical to pear-shaped, approximating to the shape of Helmholtz's resonators, and by this means a pure tone was obtained. The blast was obtained from a fan, the wind from which passed through a regulating bellows with automatic-valve action, and it was found that great care was necessary on this point. The pressure in the bellows was 2·5 inches of water, and in the speaking mouth in every case very small.

The temperature was observed by means of a thermometer entering the tube, so that the actual temperature during vibration might be recorded. The wet-bulb temperature and barometric pressure were also taken for moisture correc-

tion. The pitch was taken from a carefully tested Koenig fork of 256 vibrations, and the tubes were set to give a beating rate of about 4 per second, the lengths being read by a micrometer and standard rods. All the notes were exceedingly feeble, the pressure in the mouth being less than $\frac{1}{10}$ inch of water, much under the lowest which Regnault found to influence the velocity.

The results are given in the following Table:—

TABLE III.

Velocity of Sound in dry air, at 0° C., in tubes.

Diameter of tube	11·7 millim.	19·5 millim.	32·5 millim.	54·1 millim.
		327·09	328·72	329·90
		327·14	328·74	329·82
		326·98	328·78	329·84
		326·70	328·72	329·70
		327·09	328·72	329·95
		326·69	328·89	329·80
		326·99	328·76	329·53
		326·79	328·84	329·56
		326·70	328·84	329·65
		326·85	328·83	329·48
Means	324·56	326·90	328·78	329·72
Differences ...		2·34	1·88	0·94

I found it very difficult to get observations with the tube of 11·7 millim. diameter. Very many trials were made; and although the best single observation gave 324·78, I have reason to think that the mean value 324·56 is too high. I hope yet to verify it, and also to continue experiments with a tube 90·2 millim. diameter.

There is one point connected with the velocity of sound which appears to require elucidation, and that is, the modification it may undergo near its point of origin; for the waves which affect us as sound are usually not plane-waves, but emanate from an origin which more or less nearly approaches a point—this point being the centre of a system of spherical waves.

We may refer to the vibration in conical tubes as bearing upon this point. A complete cone, speaking its lowest note, with its apex or closed end for the position of the node, is of twice the length of a closed cylindrical tube of the same pitch,

and has the same succession of harmonics as the open cylindrical tube (see Phil. Mag. August 1878). This property is independent of the proportion of base to height in the cone; and if we assume the base of the cone to be a portion of a spherical surface described from the apex, we may continually increase the conical angle until the cone becomes a sphere. Now, if we consider the effect of this principle and apply it to divergent waves, it will be found that such waves cannot have exactly the same velocity for all pitches, but that the lower the pitch the greater will be the velocity, owing to the difference of velocity between the first and succeeding quarter-wave lengths. To take a numerical example, we may choose two wave-lengths of 32 feet and 8 feet respectively:—

	Quarter-wave lengths. Cylinder.	Cone.	Excess in Cone.
32-ft. wave . . .	8 ft.	16 ft.	8 ft.
8-ft. wave . . .	2 ft.	4 ft.	2 ft.
	Difference		6 ft.

Or, if we assume a velocity in free air (away from origin of vibrations) of 1120 feet per second, we shall have:—

Velocity of sound-wave of any pitch, not including first quarter-wave	1120 ft.
Velocity of 8-ft. wave, or wave of 140 vibrations measured from apex of cone, 1120 + 2 . . .	1122 ft.
Velocity of 32-ft. wave, or wave of 35 vibrations measured from apex of cone, 1120 + 8 . . .	1128 ft.

We thus find that waves near their origin are not of normal length.

In gun-fire experiments the pitch of the explosion is not known, and there may therefore be a variation in the recorded velocities which is due simply to an effect of conical or spherical divergence. If, in addition to the corrections made by Le Roux for temperature, some of the gun-fire experiments were submitted to a correction of this nature, the discrepancies would probably be much reduced.

XXXVII. *On the Purification of Mercury by Distillation* in vacuo. *By* J. W. Clark, *Demonstrator of Physics in University College, Liverpool.*

[Plate XIV.]

The usual processes for the purification of considerable quantities of mercury may be roughly classed as (i.) chemical (*e. g.* treatment with dilute $NO_2(HO)$, $CrO_2(HO)_2$, Fe_2Cl_6, $SO_2(HO)_2$, and $SO_2(HgO_2)$, &c.); (ii.) mechanical (*e. g.* shaking, filtering through wash-leather, &c.); and (iii.) distillation. The last-named process may be conducted either *in vacuo* or under the ordinary pressure.

Of these processes distillation *in vacuo* is in all respects the simplest and most satisfactory. Preparatory to distillation the mercury may be advantageously filtered through a writing-paper cone with a very small orifice at the apex; and when considerable quantities of lead or zinc are present, the distillation *in vacuo* may be hastened by their previous removal by one of the usual chemical methods. It is stated that the presence of $\frac{1}{10000}$ part of lead reduces the amount of mercury distilled in a given time from 67 to 55*. Gold, iridium, silver, copper, tin, nickel, cadmium, and arsenic do not influence the rate of distillation†.

The distillation of mercury under the ordinary pressure is too inconvenient a process to be ordinarily used in laboratories; not so, however, at a temperature of 180°–200° C. *in vacuo.* The first apparatus for this purpose was described by Weinhold‡, and since then Weber§ and A. W. Wright‖ have described other forms. The form shown in section in fig. 1 (Plate XIV.) differs from all the preceding chiefly in being supplied with the mercury to be distilled from a movable reservoir (in the form of a *constant level regulator*, fig. 2), the raising of which fills the distiller with mercury, which thus renders a Sprengel-pump unnecessary to set it in action. It is hoped moreover that its simplicity and efficiency and the ease with which it can be made may render its description useful.

* Gmelin-Kraut, *Hdb. der Chemie*, Bd. iii. Abth. i. S. 740, 6te Aufl.; Millon, *Ann. Chim. Phys.* [3] xviii. p. 337.

† Gmelin-Kraut, *loc. cit.*

‡ *Progr. d. k. Gewerbsch. zu Chemnitz. Rep. für Physik*, Bd. xv. S. 1.

§ *Ibid.* Bd. xv. S. 52.

‖ Chem. News, 1881, p. 311.

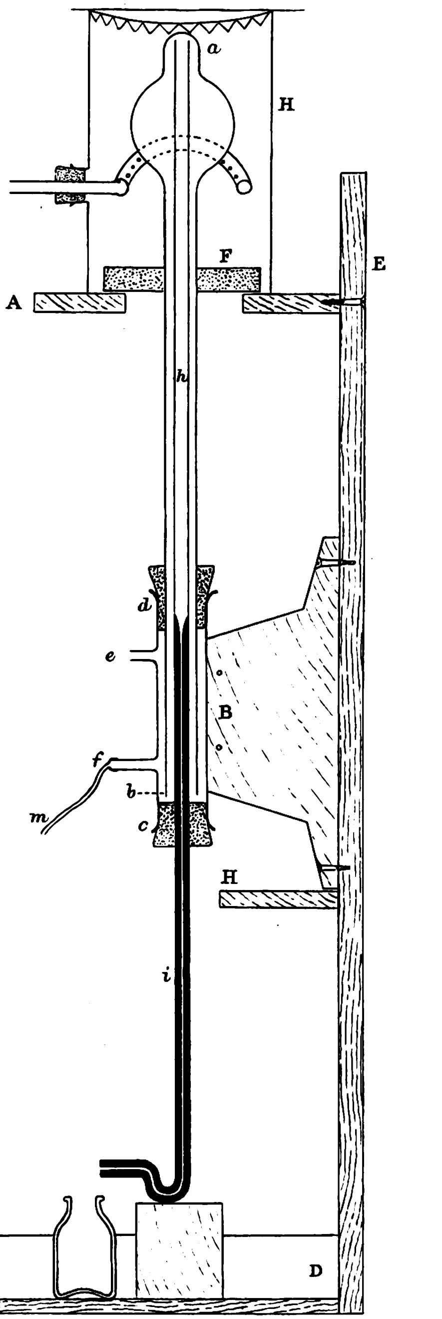


Fig. 1.

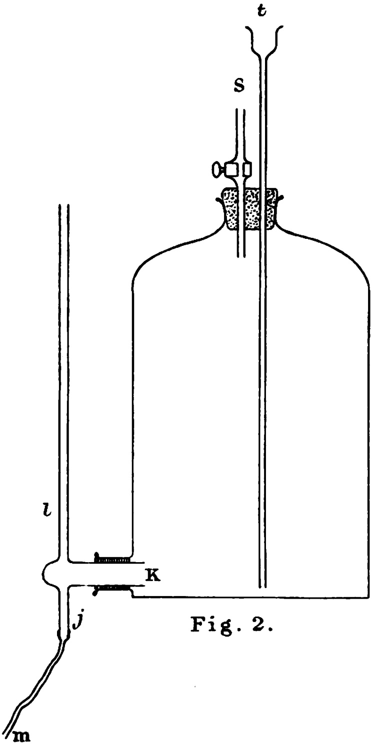


Fig. 2.

Mintern Bros. lith.

The distiller consists of a lead-glass tube, *a b* (fig. 1), 36 inches long, and about $\frac{3}{8}$ of an inch in internal diameter. A bulb of about two inches diameter is blown two inches from its closed upper end. The lower end passes air-tight through a well-secured india-rubber cork which closes the top of the cistern *d c*, and terminates at *b* a little below the tube *f*. The cistern (*d c*) is made from a piece of glass tube 1 inch in diameter and from 8 to 12 inches long, with two short pieces of quill-tubing, *e* and *f*, sealed into it. The lower end is also securely closed with a cork through which passes a piece of ordinary Sprengel-tube, *i*, 36 inches long, with a piece of quill-tubing, *h*, about 24 inches in length, fused onto the upper end. The top of this tube is nearly in contact with *a*. The internal diameter of the Sprengel-tube should not much exceed 1 millim., and the bend at its lower end is best when not much more than one inch in radius. Instead of india-rubber corks, ordinary corks soaked in melted paraffin or covered with sealing-wax may be used, but the apparatus then loses in flexibility.

The base of the stand consists of a wooden tray, C D, from which rises a stout board, D E, carrying a shelf, A E, perforated in the centre with a hole of sufficient size to allow the glass bulb to pass through it. In the Physical Laboratory of University College the board D E which carries the distiller is fixed to the wall over the mercury table. This renders the tray C D unnecessary. A large cork, F, is bored with a hole of rather less diameter than the tube *a b*, and the cork is cut in halves. By placing the tube in the position shown in the figure and twisting a piece of copper wire round the periphery of the halves of the cork, the tube is firmly supported on the shelf. The cistern is secured by string which passes through holes in the projecting piece of wood, B. A block of wood may be placed as a support for the end of the tube *i*. A tin cylinder, slightly notched round the top and covered with a flat tin plate, keeps the bulb surrounded with hot air, whilst a mica window at the side allows the height of the mercury in the bulb to be easily seen. The pipe of the brass ring-burner passes through a hole in the tin case. The diameter of the ring is about half an inch greater than that of the glass bulb, and on the inner side it has a *large number of very small holes.*

The constant-level reservoir (fig. 2) is made from a large bottle provided with a tubulure at the side. Into this passes (through a cork if the tubulure be sufficiently wide—if not, cemented in with sealing-wax) a glass tube, K, about 3 inches in length and ½ inch in diameter. Its outer end is closed, and into the upper and under sides are sealed two pieces of quill-tubing, *l* and *j*. The top of the upper one is open, but the lower (*j*) is connected with the cistern of the distiller by a narrow piece of india-rubber tubing, *m m*, about 3½ feet long, enclosed in a canvas tube. By means of an india-rubber (or paraffined) cork the thistle-funnel and small glass stopcock are fitted *air-tight* into the neck of the bottle*. Thus fitted, the reservoir is placed on an ordinary adjustable table-stand on the shelf H (fig. 1). To set the distiller in action, open the stopcock, S, of the reservoir and pour some of the mercury to be distilled through the thistle-funnel, *t*, into the reservoir, and with a short piece of india-rubber tubing and glass rod *securely* close the tube *e* (fig. 1) at the top of the cistern. Then raise the reservoir. The mercury gradually rises in the cistern, and by compressing the air in the upper part is forced up the tube *a b*, and then filling the bulb sprengels down the tube *h i*. The reservoir may then be lowered on to its stand on H and the india-rubber stopper removed from the tube *e*. The reservoir is set in action by attaching a piece of india-rubber tube to the stopcock S and sucking out air until, passing down the tube *l*, it bubbles up through the mercury in the reservoir. Then close the stopcock, and adjust the reservoir at such a height on its stand that the mercury is nearly at the top† of the bulb in the distiller. If needful, a little more air is sucked out of the reservoir, as before described. Thus set in action, the level of the mercury in the cistern *c d* will be retained constant until almost the

* So perfectly does this form of constant-level cistern work, that it seems probable that it may prove useful for other purposes—*e. g.* keeping a Sprengel-pump in uniform action for many consecutive hours &c.

† The vapour-tension of the hot mercury will depress the level in the bulb. The extent of this depression is somewhat dependent upon the height of the gas. On this account a simple form of gas-pressure regulator may be advantageously used with this apparatus. In another connection I hope to describe a pressure regulator of very convenient construction for this and other purposes.

whole of the contents of the regulating reservoir have been distilled.

To start the distillation, remove the tin plate which covers the cylinder (H) and light the gas. Five to ten minutes later, sufficient mercury will have distilled over to have entirely displaced the impure mercury originally present in the narrow Sprengel-tube *i*.

The reservoir can be replenished with mercury without interrupting the distillation. For this purpose it is only necessary to place a screw pinchcock on the india-rubber tube leading to the cistern of the distiller, open the stopcock S, and pour the mercury into the reservoir through the funnel *t*. Then suck a few bubbles of air out of the reservoir, as before described, close the stopcock, and release the screw-clamp from the india-rubber tube. The level of the mercury in the distiller will remain as before.

When it is desired to empty the distiller of mercury, air must be introduced into the bulb either by alternately sucking and blowing through a piece of india-rubber tubing connected with the end of *i*, or by disconnecting the india-rubber tube leading from the reservoir, emptying the cistern *c d*, and cautiously inclining the distiller until small bubbles of air enter at the end of the tube *a b* and rise into the bulb. The mercury then sinks into the cistern, and may be withdrawn through the tube *f*.

The Sprengel-tube *i* should be carefully cleaned and dried before putting the apparatus together.

The first time of using the distiller, it is interesting to observe that, as the tube *a b* gradually becomes heated, the surface air-film detaches itself from the glass and rises into the bulb.

The quantity of mercury distilled by an apparatus of the size and form here described is about 2 lb. an hour: very little gas is used, as the flames should be less than a quarter of an inch high and never allowed to come in contact with the glass bulb. For commercial purposes, an iron mercury-bottle and iron gas-pipe might advantageously replace the glass bulb and tube.

It seems to be possible that the latter modification of the method may be applicable to the distillation of some other metals, such as zinc, magnesium, cadmium, arsenic, &c.

XXXVIII. *On a Method of determining experimentally the Constant of an Electro-dynamometer.* *By* A. P. Chattock*.

The practical importance attaching to the accurate measurement of electric currents is daily increasing with the extended use of electricity. For this reason I venture to hope that the following account of some experiments on the calibration of an electro-dynamometer will prove interesting.

The twisting moment with which the fixed coil of an electro-dynamometer acts on the suspended coil is proportional to the product of the strengths of current flowing through the two coils respectively, and to a factor which depends on their geometrical relations.

If a current be sent through the fixed coil and the suspended coil be allowed to rotate, an E.M.F. is set up in the latter which is at any instant proportional to the speed of rotation, to the strength of the current in the fixed coil, and to a factor depending on the geometrical relations of the two coils.

By allowing the suspended coil to rotate about its axis of suspension, and by making the geometrical factor during rotation the same as afterwards, when the coil is suspended in its place, I have determined the constant of the instrument upon the table.

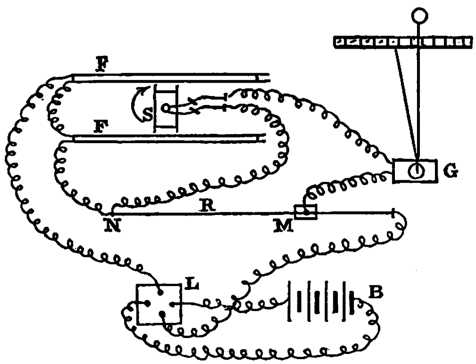
It is the method I employed in doing this which I have the honour of bringing before you to-day. The diagram shows the arrangement of the apparatus.

F, F are the fixed coils of the dynamometer; S the revolving coil to be afterwards suspended within them, and which makes contact once in every revolution with M and N; B a battery sending a current C through the coils F by the commutator L. G is an astatic mirror-galvanometer of about the same resistance as the coil S; and R is the resistance between M and N, adjustable by the slider M.

Let i be the intensity of the magnetic field, at the place where S revolves, due to unit current in the fixed coils F; H the horizontal intensity of the earth's magnetism at the same place; σ the effective area of the coil S; and k a factor depending on the relative positions of the two coils when the

* Read November 24, 1883.

revolving coil is in contact with M and N through G. $i\sigma k$ is then the geometrical factor referred to above.



Suppose the coil S rotates with an angular velocity ω, while a current of strength C flows through the fixed coils. An E.M.F. is set up in S equal to

$$Ci\sigma\omega k + H\sigma\omega k'.$$

If the resistance R between M and N be now adjusted till no current flows through G, this E.M.F. must be equal to the difference of potential between M and N due to the current, viz. CR; that is,

$$Ci\sigma\omega k + H\sigma\omega k' = CR.$$

Commutating the current at L and adjusting R, we get

$$-Ci\sigma\omega k + H\sigma\omega k' = -CR';$$

and from these two equations,

$$i\sigma k = \frac{R+R'}{2\omega}.$$

If, as is desirable, the instrument is set up so that the term depending on the earth's magnetism vanishes, we have simply

$$Ci\sigma\omega k = CR;$$

and the value of $i\sigma k$ being obtainable from one reading, there is no error due to variation of the current.

Having thus obtained the value of $i\sigma k$, the coil S is suspended within the coils F so that its axis of suspension coincides with its previous axis of rotation; and by means of sights, properly adjusted, it is brought into the same position relatively to F as it occupied when making contact with M and N.

If now an unknown current C be made to flow through the instrument, and the electrical turning moment of the suspended coil be opposed by an equal and opposite mechanical moment T in the suspension-springs, we have

$$T = C^2 i\sigma k + HC\sigma k';$$

and on commutation of the current,

$$T' = C^2 i\sigma k - HC\sigma k',$$

from which

$$C = \sqrt{\frac{T+T'}{2i\sigma k}} = \sqrt{t+t'}\sqrt{\frac{T_0}{2i\sigma k}} = \sqrt{t+t'}\,K;$$

where T_0 is the moment due to unit twist on the springs, and t the actual twist on them.

Here, as before, the instrument can be set up so that $HC\sigma k'$ vanishes, and a value of the current can be obtained in one reading.

The absolute value of T_0 can, of course, be found accurately by taking the time of vibration of a mass of known moment of inertia when suspended in the place of the coil S; and thus the constant, K, of the instrument can be determined.

There are two points in the practical carrying out of this method which require special attention—the prevention of thermal and other extraneous currents in the circuit of the revolving coil, and the keeping its speed uniform throughout each revolution. Success in both these cases depends chiefly on the manner of making contact between the revolving coil and the points M and N.

In my first experiments with the instrument I allowed the two ends of the revolving coil to touch, in passing, two fixed wires in connexion with M and N. After calculating a constant, I compared the instrument with the very accurate tangent-galvanometer belonging to University College, and found a difference of 6 per cent. between them, the readings on mine being the lower of the two.

Thinking that this discrepancy might be due to the speed at the moment of contact being greater than the average speed (which was all that could be measured), and that this difference,

again, might be due to the friction of the contact (as well as of another contact used for counting the revolutions), I substituted contact-rollers with platinum rims for the fixed wires. The axles of these rollers were vertical, and were furnished at their upper ends with mercury-cups into which dipped the wires connected with M and N. This was to avoid thermal currents. The rollers were pressed against the rim of an ebonite disk, fixed coaxially to the revolving coil. Into this rim were let two pieces of platinum, connected with the ends of the coil and making contact with the rollers as they passed them. The result of this alteration was that the difference between the readings of my instrument and the tangent-galvanometer was reduced to 1·5 per cent.

I have not been able to experiment further in this matter; but I believe that, by substituting for the rollers an arrangement of light levers, by which a bent wire could be lifted in and out of two mercury-cups with each revolution of the coil, thereby making and breaking contact, a still greater degree of accuracy should be attainable. The addition of a fly-wheel to the coil while revolving would also tend to keep the speed uniform.

A partial explanation of the disagreement of the two instruments may be found, too, in the fact that mine is home-made, and therefore not so well finished as it otherwise might be.

With the aid of a tangent-galvanometer of known constant, the dynamometer can be calibrated to give a convenient absolute measure of the horizontal intensity of a magnetic field. For this purpose the axis of the suspended coil must be at right angles to the direction of the magnetism.

The instrument is first set up in such a position that on sending a current through the suspended coil alone, no deflection is produced. It is then turned bodily through a right angle by means of a graduated foot, and in this position it is connected in series with a tangent-galvanometer and battery.

Let H be the strength of the field at the place where the dynamometer stands, and H′ the same where the tangent-galvanometer stands. Then, for a current C sent through both

instruments and commutated, we get from the dynamometer the two following equations:—

$$\mathrm{T} = \mathrm{C}^2 i\sigma k + \mathrm{HC}\sigma,$$
$$\mathrm{T}' = \mathrm{C}^2 i\sigma k - \mathrm{HC}\sigma;$$

from which

$$\mathrm{CH} = \frac{\mathrm{T}-\mathrm{T}'}{2\sigma};$$

and from the tangent-galvanometer we have

$$\frac{\mathrm{C}}{\mathrm{H}'} = \frac{\tan\theta}{i'}.$$

Interchanging the two instruments and repeating the experiment, we obtain two similar equations with H and H′ interchanged. From which

$$\sigma = \frac{1}{2\mathrm{CC}'i'}\sqrt{(\mathrm{T}-\mathrm{T}')(\mathrm{T}''-\mathrm{T}''')\tan\theta\,.\,\tan\theta'},$$

where every thing is known, the values of C and C′ being determinable by the dynamometer from the same readings. σ being once known, the horizontal intensity of a magnetic field is determinable by simply setting up the dynamometer so that the axis of its movable coil is at right angles to the meridian, as described above, and then sending the same current through it successively in both directions, the equation being

$$\mathrm{H} = \frac{\mathrm{T}-\mathrm{T}'}{2\sigma\mathrm{C}} = \mathrm{K}'\,.\,\frac{t-t'}{\sqrt{t+t'}}.$$

A somewhat different use of the dynamometer for the same purpose is given by Kohlrausch in his 'Physical Measurements,' p. 180.

In conclusion I wish to thank Prof. Foster for his kindness in allowing me to test my instrument in the Physical Laboratory at University College. My thanks are also due to Mr. W. S. Grant for many valuable hints on the construction of the instrument.

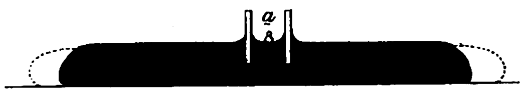


Fig. 1.

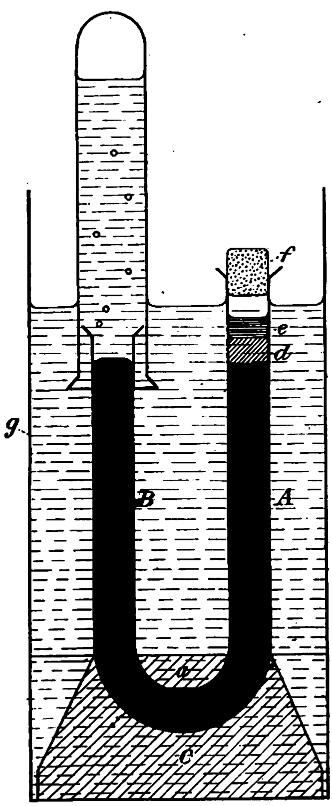


Fig. 2.

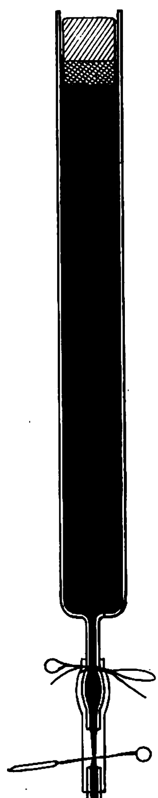


Fig. 3.

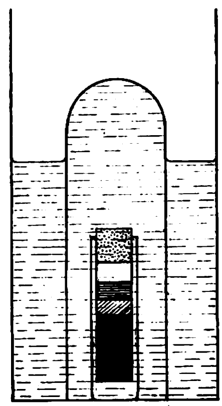


Fig. 4.

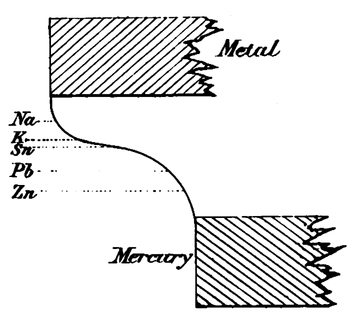
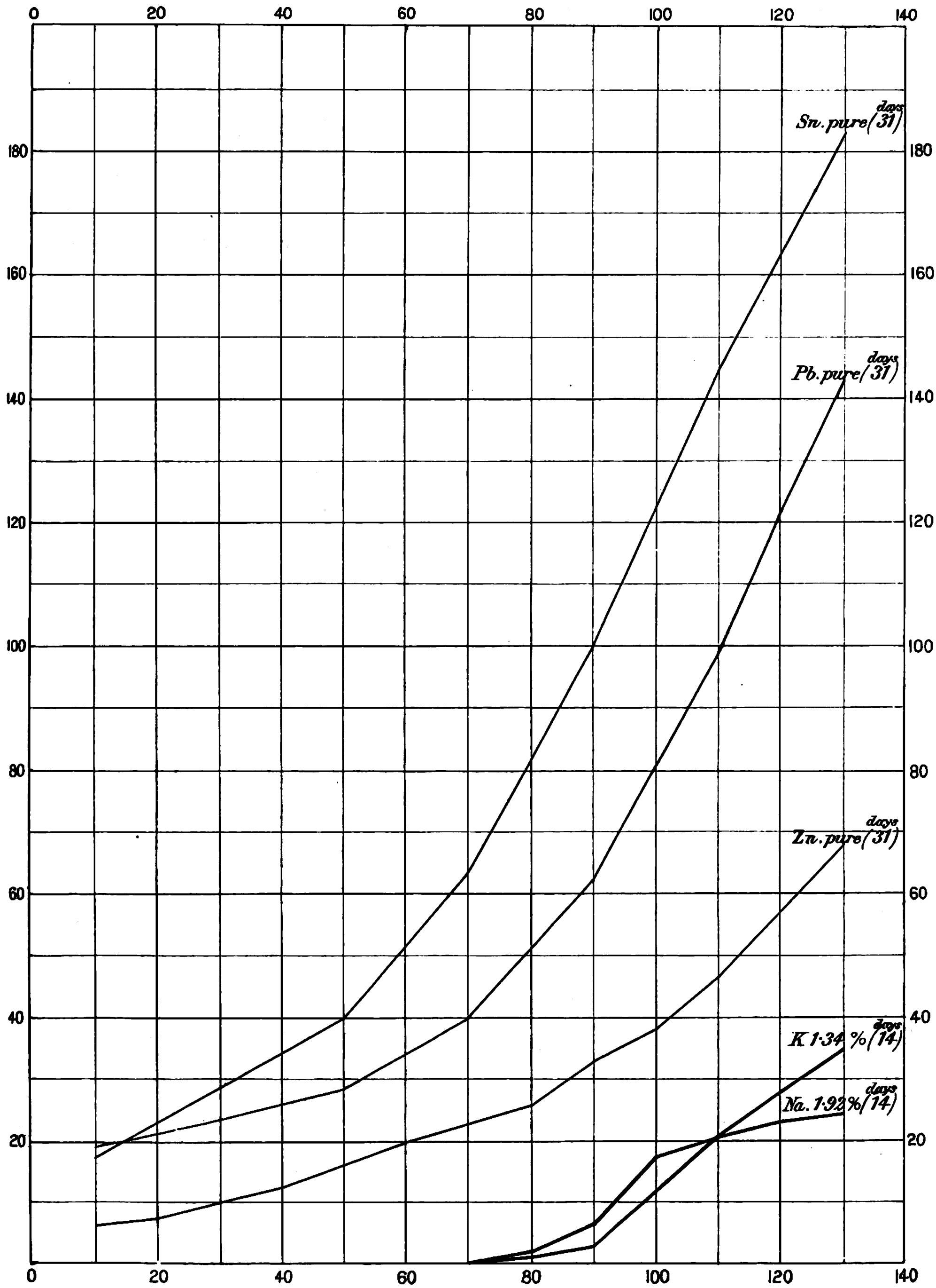


Fig. 5.

Mintern Bros. lith.



Mintern Bro's lith.

XXXIX. *On certain Molecular Constants.* *By* Frederick Guthrie.*

[Plates XV. & XVI.]

Liquid Slabs. Metallic Diffusion.

§ 1. *Liquid Slabs.*—When a little liquid is poured upon a flat horizontal surface which is not attacked by the liquid, a circular disk of liquid is formed, the shape of the edge of which has been very fully examined by Quincke and others. In most such cases, one of the most important factors is the specific relationship in the sense of adhesion between the solid and the liquid. In fact the question, like all questions of capillarity, involves density (and gravitation), cohesion, adhesion, and surface-tension. Such experiments show the relationship between two bodies as well as the physical attributes of one. About twenty years ago I made an attempt to get rid of the factor adhesion, with partial success, by examining the size of a liquid drop. But I soon found that other factors, notably the shape of the solid bodies from which the dropping occurred, and the rate of dropping, introduced arbitrary conditions which removed the measurements from the class of simple physical constants.

§ 2. The plan adopted in the following experiments is the endeavour to support a mass of liquid above a plane surface in such a way that no actual contact ensues, not even such as takes place between clean glass and mercury. If such can be done, it is clear that we shall have a circular flat slab with rounded edges, and into the shape of that slab the influence of adhesion by no means enters. If the thickness of the slab be found to be a constant, we shall have a constant as characteristic as density, and, like density, varying for the same mass only according to volume, such volume-change in our case being brought about by heat alone. Such slab-thickness has for its negative influence the action of gravity (density), for its positive the cohesion and surface-tension.

§ 3. The actual measurements of the slab-thickness I have performed in two ways :—(1) by a spherimeter which, when used as such, gives results trustworthy to the $\frac{2}{10000}$ of an inch.

* Communicated to the Physical Society during the Session 188283.

But the upper of the two surfaces whose distance has to be measured being liquid, and the lower one not very hard, the spherimeter cannot be used by the method of touch. Accordingly I have measured the slab-thickness indirectly. A known volume of the liquid is poured on the surface, and teased into the circular form if it shows any noticeable departures from it. Four or five diameters are measured by means of a small horizontal cathetometer. The mean being taken, an allowance has to be made for the meniscus. This reduces the shape to the cylindrical, from which the thickness h is deduced by means of the equation

$$h=\frac{V}{\pi r^2}.$$

§ 4. In regard to the actual apparatus:—Upon a thick round slab of paraffin, a foot in diameter and 4 inches thick, a massive foot of plaster is cast. The whole is placed on a three-screw levelling support. The surface of the paraffin is scraped into a true plane. When water was being examined, the surface of the paraffin was lightly powdered with lycopodium and the water poured on vertically from a fine opening. With some care a perfectly round slab of water 6 inches in diameter can be formed, which is so free to move that the greatest nicety of adjustment in the levelling-screws is necessary. Precisely the same arrangement can be adopted for mercury. But it was found that for the latter liquid a sheet of blotting-paper wetted and allowed to dry on a sheet of plate-glass gave results identical with those of the paraffin surface. The paper surface was used in some of the experiments. As to the allowance for the meniscus, it is clear that this is of less consequence with large slabs than with small ones. Indeed, with slabs a few inches in diameter the meniscus might be neglected. This was imperfectly shown in the case of mercury by adding exactly equal volumes to a small slab. After the slab had passed 2 inches in diameter, each additional volume produced a "parabolic" increase in the diameter. Data derived from this and from the measurement of an enlarged photograph of the edge gave me as a mean 2 millim. to be deducted from the diameter in the case of mercury. Assuming it to be the same for water,

the error incurred, after making this reduction, could not in a 6-inch slab be more than $\frac{1}{500}$ of the diameter. This would be negligible in the deduced thickness.

§ 5. I give the following datum for mercury on account of the accidental coincidence of the experimental numbers with numbers easy of remembrance, excepting as to the temperature, which is, however, not far from the conventional temperature of 60° F.

100 cub. centim. of mercury at 14° C. has an extreme radius of 100 millim.

$$h=\frac{100,000}{3{\cdot}1416\times 99^2};$$

thickness of mercury slab $=3{\cdot}248$ millim.

In the case of water it was found so difficult to get a nearly circular slab with 100 cub. centim., that only 50 were employed. The slab may then be teased into a circular form by means of a stick of paraffin covered with lycopodium.

50 cub. centim. of water at 14° C. has an extreme radius of 54·8 millim.

$$h=\frac{50,000}{3{\cdot}1416\times (53{\cdot}8)^2};$$

thickness $=5{\cdot}50$ millim.

Glycerine * is a beautiful liquid in this respect. It is kept off from the paraffin surface by a very faint blush of lycopodium, and it travels very slowly. It can be got into a circular slab more easily than water; but, perhaps on account of its capillary action towards its lycopodium props, it is more persistent in its motion. In fact, unless there be hills of that substance to confront it, it rolls along (for that is the motion of a slab however large) and forms a "level," which requires a very steady support to avoid the notion that its motion is affected by the gravity of the observer.

50 cub. centim. of glycerine at 14° C. has an extreme radius of 59 millim.

$$h=\frac{50,000}{3{\cdot}1416\times (58)^2};$$

thickness $=4{\cdot}731$ millim.

* Commercial, "Price's."

§ 6. Accordingly, taking the slab thickness of water as unity, we may begin a table which will at some future time assuredly be extended.

Specific Slab-thickness (at 14° C.).

Water .	=1·0000,
Glycerine	=0·8602,
Mercury	=0·5906,

These numbers may be, with instruction, considered in reference to the numbers in table vii., which concern the drop-sizes of the same three liquids, in the 'Proceedings of the Royal Society,' 1864, p. 17 [" Recess "]. It will, I have no doubt, appear that in all cases the greater the drop-size the greater the slab-thickness. Water will, no doubt, again assert its singularity and exhibit the greatest slab-thickness.

§ 7. Restrained as slabs are in their form by skin-tension as well as cohesion, it is found that the addition of a liquid which diminishes the former diminishes also the slab-thickness. Taking 25 cub. centim. of water at 14° C., a slab was formed having 38 millim. corrected radius. This gives a thickness of 5·51 millim. Such a slab is unchanged if touched in the middle by a drop of glycerine. But on touching it with " glacial " acetic acid, it instantly acquires a corrected radius of 44 millim., or thickness of 4·16 millim. This means a diminution in thickness of very nearly 25 per cent., or one quarter. The question therefore presented itself, What is the slab-thickness of " glacial " acetic acid?

I reserve the results of my experiments in the direction of the relationship between the liquids and the alteration of skin-tension.

§ 8. The mercury slab, like the water slab, has what virtually amounts to a skin; and it became interesting to examine the conditions of this skin or region of surface-tension. If lycopodium be strewn upon the surface of a mercury slab, and a little tin, zinc, or lead, or amalgam of these metals be made to touch the slab in the middle, no noticeable disturbance takes place. But if such a slab be touched by an amalgam of K or Na, the slab instantly expands, and the film of lycopodium-powder on its surface cracks radially, exposing the

brilliant metallic surface, which is seen to be agitated over its whole extent. In a few seconds the slab contracts to its original size and the lycopodium cracks heal.

Does this extension of the slab depend upon the diminution of the cohesion of the mass of the mercury, or upon a surface effect?

§ 9. I frequently in my researches have had recourse to the fact, which I first described in the year 1863, that a little sodium added to mercury enables that metal to touch with positive capillarity metals which in its and their ordinary state are not wetted by the liquid metal. I here make use of the same fact. A platinum tube (fig. 1, Pl. XV.), 6 millim. in internal diameter and 2 centim. in height, is rubbed and soaked in some weak sodium amalgam, and then washed in several quantities of pure mercury. Placing such a tube vertically in the middle of a slab of mercury so that its lower edge is clear of the surface upon which the mercury slab rests, we have the condition shown in fig. 1. A little grain of sodium amalgam dropped into the platinum tube causes no immediate change; but in a time measurable by seconds, say 20 to 30 seconds, the slab starts on its expansion and reaches its maximum size, apparently immediately. It seems, then, that since the effect is not instantaneous, it is a surface effect. The effect when produced is due to an alteration of the surface between the tube and the outer portion of the slab. By dipping the platinum tube further down into the slab so as to be within $\frac{1}{50}$ of an inch of the bottom, I have found the effect to be distinctly delayed.

§ 10. The fact mentioned in § 9, that the release of the mercury skin-tension by sodium is brought about after a time, short indeed, but appreciable when introduced into the central part of a liquid slab inside the platinum tube, points to the existence of a true diffusion between the metals; and this leads to the second part of this communication. For I have examined already a few such cases, which I will now describe, because I believe the subject of elementary diffusion has been neglected excepting in the case of gases, and even here but little is really known.

§ 11. *Metallic Diffusion.*—The metals potassium and sodium suggested themselves of course at once. They offer excep-

tional facilities for the determination of the composition of the mixture, when they have diffused through mercury, because the mere addition of water translates the alkaline metal into hydrogen. The neutralization of the alkalized water, say, by hydrochloric acid, and subsequent evaporation and weighing, give a control upon the hydrogen translation of the alkaline metal. The mercury is thereupon left nearly ready for weighing.

On the other hand, I have not yet been able to establish a column of mercury having an unlimited stock of pure cold alkaline metal above pure mercury at the same temperature below. I do not see the possibility of it. Granted that when such metals as tin, or lead, or gold, or silver dissolve in mercury, heat may move, such movement of heat is, I should think, swamped in its power of causing convection-currents by the conductivity of the mass. But in the case of the alkaline metals the first contact of the two metals is accompanied by so much heat that the conditions obtainable with other metals are here far more difficult. Perhaps mercury and sodium brought into contact at a temperature far below the freezing-point of mercury might give the required starting-point. If their contact were real and the elevation of temperature very gradual and well controlled, we might have a trustworthy condition; but scarcely so at a single temperature.

Such a condition would represent a certain fixed sodium potential (not infinite, because the sodium has to be disintegrated), on the one hand, and a lower, but not zero, on the other; and between the two the integral of the resistances of the various amalgams after the first contact.

§ 12. This being so, I elected to employ sodium amalgam and potassium amalgam rather than the free metals.

On mixing sodium with mercury, the two combine with great energy and liberate so much heat as to point to a loss of volume. Is this loss of volume, if it take place under any circumstance, so great as to give rise to an amalgam having a greater density than mercury itself?

If $\frac{m_1}{v_1}$ be the density of mercury and $\frac{m_2}{v_2}$ that of sodium, and if v_3 be the volume of the amalgam, then the density of

the amalgam would be equal to that of the mercury, if

$$\frac{m_1+m_2}{v_3}=\frac{m_1}{v_1},$$

or

$$v_3=v_1\left(1+\frac{m_1}{m_2}\right).$$

If v_3 should be less than this for any ratio between the constituents, the convection-currents of sodium would at all events begin to flow down if such an amalgam were at the top of the mercurial column.

On this point, without making a study of the specific gravity of alloys of sodium of different strengths, I have satisfied myself that, as long as the amalgam is liquid, it is lighter than mercury. This is easily shown by introducing mercury into one limb and the various liquid amalgams of sodium into the other limb of a long U-tube: whereupon the pure mercury always prevails in weight. Now when a solid amalgam of sodium is brought into contact with mercury, heat may be either set free or absorbed. Chemists will understand me if I remind them that a pounds of water mixed with b pounds of chloride of calcium will give a body which will set free or absorb heat according as a is greater or less than x.

§ 13. Accordingly I made a pound or two of a sodium amalgam of such a strength as to be solid at the atmospheric temperature. This was beaten up in an iron mortar as it cooled. Putting some of this into a porcelain crucible, plunging it into water containing a few drops of hydrochloric acid, and collecting the hydrogen, it was found that after a day or two, if the amalgam was occasionally stirred, all evolution of hydrogen ceased; the volumes, reduced to dry hydrogen at 0° C. and 760 millim., were $\left\{\begin{matrix}156\\128\end{matrix}\right\}$ cub. centim. The mercury, after drying, was found to weigh $\left\{\begin{matrix}15{\cdot}1096\\13{\cdot}7841\end{matrix}\right.$. This gives the percentage of the amalgam, which I shall call A*m* amalgam :—

Hg.........	98·2	97·97	98·08
Na.........	1·8	2·03	1·92
	100·0	100·00	100·00 (mean)

The ideal amalgam would perhaps be one of such a composition that heat would neither be set free nor absorbed on

further mixing with mercury. But such an ideal condition could only be ideal in its beginning, and, I think, disturbances due to this cause are insensible in comparison with other sources of error. The above amalgam, when stirred with mercury, may reduce its temperature as much as 5° C.

I am informed that sodium may contain a large quantity of hydrogen. I am not called on to discuss the experiments (not my own) upon which this rests; but I think that any considerable quantity would be expelled on amalgamation. Perhaps the glow or blush to be described immediately and in § 14 is due to the escape of residual hydrogen at the released tension-surface of the mercury.

The first experiment in regard to the diffusion of sodium out of this amalgam into mercury was of course a qualitative one. A U-tube (fig. 2, Pl. XV.) was made of glass tube of $\frac{1}{2}$ inch internal diameter—the one limb, A, being about 3 inches and the other, B, about $2\frac{1}{2}$ inches long, reckoned from the inner bend *a*. This was fastened into a massive fusible metal foot to give stability. The U-tube was dried perfectly under the ordinary air-condition, and received pure dry mercury, which stood in both limbs at a height of about $2\frac{2}{5}$ inches (reckoned from *a*). The whole was placed in a flat-bottomed vessel *g* containing a little melted paraffin, and then upon an immovable slab, to which it was stuck by a few drops of paraffin. The vessel *g* then received water slightly acidulated with HCl so as to cover the mercury in the shorter limb, and reach about $\frac{1}{4}$ inch above the edge of the glass tube on that side. A test-tube filled with similarly acidulated water was inverted over the shorter limb. Upon the surface of the mercury in A about 15 grams of the amalgam A*m* was placed; this was covered with petroleum, and the tube was plugged with cotton-wool.

Immediately after introducing the sodium amalgam a kind of frosted appearance is seen on the immediately lower parts of the mercury and glass surface in A. This appearance, which is a blush of bubbles, creeps downwards with strange rapidity, reaching the bend, say $2\frac{4}{10}$ inches, in a quarter of an hour.

In about 30 hours, bubbles of hydrogen appear at the surface of the mercury in B and collect in the pneumatic tube. Such evolution continues sensible for about a month. After two months such evolution ceased, the contents were emptied

out, thereby being of course mixed, and no further evolution of hydrogen could be detected.

Such a method of experimentation is, however, far from quantitative, because when the sodium has diffused down through A as far as *a*, it will, being lighter than mercury, rise through B and cause whirls.

The ideal condition of such diffusion would be of course similar to the ideal condition of heat- or electrical transference, where one may have a given potential at one end of the column, and a given lower one, fancifully called zero, at the other.

Perhaps this condition is to be attained with the greatest practical completeness by the simple *long* vertical column.

§ 14. Three glass burettes were made, about a foot in length and an inch in internal diameter. They were drawn out sharply at the bottom into capillary tubes, upon which pressure-taps were fixed in the ordinary way. These were nearly filled with pure mercury. A little of the mercury was allowed to run through so as to fill the capillary and caoutchouc tube.

Upon a tube so prepared and filled, about 15 grams of the amalgam A*m* were placed. The amalgam was thereupon covered liberally with petroleum; and the top of the tube was slightly corked. Instantly clouds of minute bubbles began to make their appearance between the mercury and the glass. In half an hour the whole column appeared frosted (see § 13). On drawing off a measure, say $\frac{1}{13}$ of the whole, from the bottom after two or three hours, no appreciable amount of hydrogen was to be got from it.

Accordingly the tube was reemptied, cleaned, dried, and refilled. The amalgam (A*m*) was then allowed to rest upon the top for 14 days and nights in an undisturbed and steady place, where the temperature ranged from 13° to 18° C. At the end of this time the amalgam was drawn off. The drawing off was effected as follows:—A little block of paraffin was hollowed so as to have a smooth cavity of the capacity of about $\frac{1}{13}$ of the tube in fig. 3. The edge was ground flat, and a flat slab of paraffin served as a cover. The amalgam was drawn into this very slowly so as to stand above the edge; the slab being then pressed down, a unit volume was entrapped. This being transferred to a porcelain capsule, the few drops of

overflow were returned to unit measure, which was again filled up, and so on. The six lowest measures (each about $\frac{1}{13}$) did not show a trace of hydrogen. The seven higher ones evolved hydrogen in the quantities shown in the following table, in which the actual weights of the mercury are reduced to 100, the cub. centim. of hydrogen being recalculated and reduced to dry hydrogen at 0° C. and 760 millim. It appears that in 14 days the sodium had penetrated down a little more than halfway, say 7 inches, in quantity appreciable.

I put now these results in such a form that they may be, as far as possible, immediately comparable with the results obtained by other metals. They come out as follows :—

Per cent. Na.	Hg.		Na.
·0035…………	100	and	·0035
·0178…………	„	„	·0178
.0665…………	„	„	·0666
·1769…………	„	„	·1772
·2034 …………	„	„	·2038
·2295…………	„	„	·230
·2414…………	„	„	·242

§ 15. A potassium amalgam prepared in a similar manner was found, when analyzed as in § 13, to have the composition 1·34 per cent. of K. About the same quantity of this was put into the same tube as had been used for the Na, under, as far as possible, the same conditions.

Reducing the evolved. H to 0 and 760, as before, it was found that the 13 volumes of the column (all of which were nearly equal except the last, which, instead of about 84–82 grams of mercury, only held about 52, for this was the drainage from the amalgam), had the composition :—

Per cent. of K.
0·00082
·0038
·0146
·0331
·1185
·2061
·2811
·3490

As to the comparison between Na and K, we need only contemplate the potential-difference between 1·92 and 1·34 respectively.

With regard to the frosted appearance mentioned in §§13, 14, it can scarcely be doubted that the minute bubbles which compose it are hydrogen, due to the film of water or vapour on the glass. But while this appearance travels at the rate of at least one foot an hour, there is no sensible quantity of Na to be found at even a lesser depth after fourteen days. The effect must therefore be a surface-effect, and be of the same order as the effect described in § 8, where the mercury-slab expands when touched by sodium amalgam, on account of metals spreading almost instantaneously over its surface and enfeebling its skin. The condition actually set up in the mercury column is probably this :—A minute film of sodium spreads downwards between the mercury and the glass : this decomposes the water on the glass, and so clothes the glass with a film of minute hydrogen bubbles, and the mercury surface with a film of caustic soda, which latter is in absolute contact with the mercury surface. It is a question whether the sodium film is less than, equal to, or more than sufficient to decompose the water—probably more. At all events it is so minute as not to exhibit itself in any chemical reaction. The spectroscopic reaction here has no significance.

The curves Na and K, Plate XVI., which represent these experiments graphically, are not directly comparable with the curves Sn, Pb, and Zn (§ 17) in the same plate, because in the case of Na and K, for reasons given in § 11, it was found necessary to start with an amalgam, and indeed with one containing only about 2 per cent. of sodium. The time in the case of the Na and K amalgams was also a little less than half that occupied in the diffusion of Zn, Pb, and Sn.

§ 16. The rapid penetration of zinc by mercury suggested the question whether, when an amalgam of an alkaline metal was presented to zinc, the mercury would penetrate the zinc and carry the alkaline metal with it. Accordingly the above potassium amalgam was introduced into a hollow cylinder of cast zinc, 17 millim. internal, and 21 millim. external diameter (thickness 2 millim.), 45 millim. external height, 35 millim. internal height (10 millim. thickness of bottom). The

amalgam was scraped upon the zinc so as to ensure contact, and then covered with petroleum. The zinc cylinder was thereupon corked up and the cork covered with paraffin. It was placed in a beaker of distilled water and covered with a tube of water according to fig. 4, Plate XV. After two months' standing at a uniform temperature of about 15°, scarcely a pin's-head volume of gas had collected in the top of the tube. Abundance of the semiflocculent fine oxyhydro-carbonate had collected on the zinc and at the bottom of the beaker. That part on the zinc was rubbed off the zinc with an ivory blade, and, together with the sediment in the beaker, dissolved in hydrochloric acid overneutralized with ammonia and sulphide of ammonium. After separation of the Zn, no trace of K could be found. No potassium had found its way through the zinc. Perhaps a more remarkable fact still is this, that on scraping about a gram of the solid metal from the outside of the zinc cylinder, not a trace of mercury could be found in it. Not only, therefore, did the alkaline metal fail to follow the mercury into the zinc, but it prevented the mercury from entering the zinc. Compare this with § 18, where the cylinder of zinc is literally "slaked" by the mercury.

§ 17. Cylinders of zinc, lead, and tin were cast, an inch and a quarter long and $\frac{7}{8}$ inch in diameter. These were floated on the mercury contained in the tubes described in § 14. The quantity of the mercury in each tube was such that it stood at the same height, reckoning from the bottom of each cylinder. The burettes had been previously lashed to massive stands, cork buffers being interposed between the tubes and the stands. The three were placed side by side on a slab let into the wall, and were protected as much as possible by cloths from sudden changes of temperature. The mean temperature was 15° C. The experiments lasted a month, and the extreme range of temperature was from 13° C. to 17°·5 C.

At the end of the month (31 days) the mercury was run off from the bottom very slowly and discontinuously into the paraffin vessel described in § 14; so that, with the exception of the top quantities, the volumes of the successive portions were very nearly the same. With regard to the top quantities, it is clear that, since the metals float at different depths in the mercury, the surfaces of contact are not the same in the

several cases; and therefore these top, or richest, amalgams can scarcely be compared. Again, the shape of the bottom of the tube with its capillary &c. puts the lowest or poorest out of court. But as the contents of the lower, irregular part of the tube is not more than a third of the volume of the unit measure, it is only the very lowest amalgam that need be rejected.

In each case there were twelve full unit vessels drawn off, and in each case a fraction of a thirteenth, which last contained the drainage from the metal.

Through the kindness of Dr. Hodgkinson a number of these amalgams were analyzed in the chemical laboratory by Messrs. Adie, Gahan, and Grange, to whom I am therefore indebted. These three gentlemen analyzed the zinc, lead, and tin amalgams respectively. The metals were determined in the following manners:—

Lead.—The amalgam dissolved in nitric acid and evaporated with sulphuric acid, and the residue either ignited directly or after washing with dilute alcohol (as sulphate of lead).

Tin.—The amalgam dissolved in nitric acid, evaporated to dryness, and ignited (as metastannic acid).

Zinc.—(α) By dissolving in nitric acid, evaporating to dryness, and igniting; or (β) by separating the mercury as sulphide and the zinc as sulphide, and igniting (both as oxide of zinc).

§ 18. In Table I. the results of such determinations are given, so that the proportion of the errors of analysis may be compared with the true diffusion in each, and the difference of diffusion in the three cases.

At the end of the experiment the cylinders of tin and lead presented nothing remarkable in appearance. On standing a couple of months the upper part of the lead cylinder has become as hard as zinc, though there is no sensible deformation. The zinc cylinder swelled considerably in the tube; and when left to itself afterwards, though drained from the mercury, it continued to swell and crack, and ultimately fell to pieces like a "lime-light" lime-cylinder when slaked. Two cones with their apices towards the centre of the cylinder were formed at top and bottom; the cracking otherwise was for the most part in radial planes.

In Plate XVI. the percentages of metal in the several amalgams of the three metals are given graphically, and without founding-off or other interpolation. The abscissæ reckoned from the left are distances from the bottom ; the ordinates are the corresponding percentages of the respective metals.

TABLE I.

	Zinc.	Per cent.	Lead.	Per cent.	Tin.	Per cent.
1 (bottom).......	0·064 0·062	0·063	0·188 0·204	0·196	0·13 0·21 0·174	0·171
2..................	0·076 0·080	0·078				
3..................	....		0·243 0·225	0·234	0·28 0·30	0·29
4..................	0·126 0·117	0·122				
5..................	....		0·303 0·274	0·289	0·38 0·41	0·40
6..................	0·214 0·183	0·199				
7..................	....		0·402 0·403	0·403	0·63 0·62	0·63
8..................	0·254 0·266	0·260				
9..................	0·337 0·338 0·325	0·333	0·609 0·646	0·628	0·99 1·01	1·00
10..................	0·408 0·365	0·387				
11..................	0·468 0·454	0·461	1·03 0·94	0·99	1·68 1·41 1·25	1·45
12..................	0·573 0·569	0·571				
13 (top).	0·618 0·735	0·677	1·38 1·47	1·43	1·86 1·87 1·76	1·83

§ 19. It appears accordingly that the three metals lead, tin, and zinc, all of which and all of whose amalgams are

lighter than mercury, diffuse downwards through this latter metal in such a fashion that they appear, after a month's interval, in appreciable quantity at a depth of a foot beneath the surface when the temperature is about 16°–17° C. With regard to this latter point, as to temperature, I suppose that mercury is so good a conductor of heat that the influence of convection-currents is at least as inconsiderable as in the experiments which have been performed for determining the diffusion of soluble salts in water. It is scarely worth while amusing oneself by dividing these diffusion *percentages* by the so-called "atomic weights" of the metals. A more serious consideration might be the result of the division of the diffused weight by the specific gravity of the metal. Comparing the numbers of the group Sn, Pb, and Zn with one another, we may remember that the metals are all cast, and therefore so far indefinite in structure. This may be especially the case with zinc, which cracks and thereby allows the mercury to rise by capillarity and so enrich itself, and generally set up conditions of amalgamation which I do not care to trace, for I do not see my way through.

As to the comparison of the alkaline group with the Sn, Pb, Zn groups, such comparison must be vague, for the reason that the K and Na are employed as amalgams (as though one would study the diffusion of nitre into water by employing a solution of nitre containing only 2 per cent. of the anhydrous salt); whereas the Sn, Pb, and Zn are used with what was supposed to be a sufficient supply of pure 100-per-cent. metal. But this imperfection of these conditions is manifest if we remember that while the solid metal melts and dissolves downwards, the liquid mercury rises. Accordingly there is, after the first instant of contact, supposing the metals diffuse, no constant metallo-motive force in the same place.

§ 20. I conclude therefore that the general curve of amalgamation, and therefore of alloyage, and therefore perhaps of elementary atomic and molecular diffusion generally, is of the kind shown in fig. 5, Pl. XV. In the case of Na almost the complete curve was obtained; whereas in the case of Zn the point of contriflexure had not been reached. The very fact that the K and Na curves are more complete in this fashion than the Pb, Zn, and Sn, prove to my mind that K and

Na have a far greater diffusive energy than the heavier metals examined. And although in this case the percentage of metal actually found at a given depth was in all cases much less than the percentages of the heavier metals, it will be borne in mind that, while the latter were pure and had acted for thirty-one days, the former were amalgams containing less than 2 per cent. of the metal. Comparing K with Na, I do not think we can draw any conclusion beyond the rather negative one, that the superior diffusive faculty which seems to be the property of K salts in regard to water does not evidence itself, if it exist, when that metal and sodium are compared in respect to their diffusion in mercury. I am far from asserting that such preeminence may not exist; but I do not think that it is here made conspicuous.

My friend Prof. Chandler Roberts has for a long time been engaged in studying the diffusion of melted metals, and the matter has been a subject of frequent conversation between us. I await with great interest the details of his experiments. The relative dates of our publication have no relation to the dates of our experiments.

XL. *On a new Insulating Support.*

By Professor Silvanus P. Thompson, *B.A.*, *D.Sc.**

[Plate XVII.]

Insulating supports consisting of rods rising through the necks of glass jars containing concentrated sulphuric acid, for the purpose of absorbing moisture which otherwise would condense upon the glass, appear to have been first introduced in practice by Sir William Thomson†. Similar devices have been more recently designed by Mascart‡, by Professor Clifton§, and by Professors Ayrton and Perry. The apparatus of Mascart differs only from the original design of Sir W.

* Read December 8, 1883.

† Proc. Roy. Soc. June 1867, and 'Reprint of Papers on Electrostatics and Magnetism,' p. 322. See also the figure given on page 14 of Maxwell's 'Elementary Treatise on Electricity.'

‡ *Journal de Physique*, t. vii. p. 217 (1878); 'Nature, xviii. p. 44 (1878); see also Wiedemann's *Electricität*, Bd. i. p. 16.

§ Proc. Roy. Soc. No. 182, p. 300 (1877).

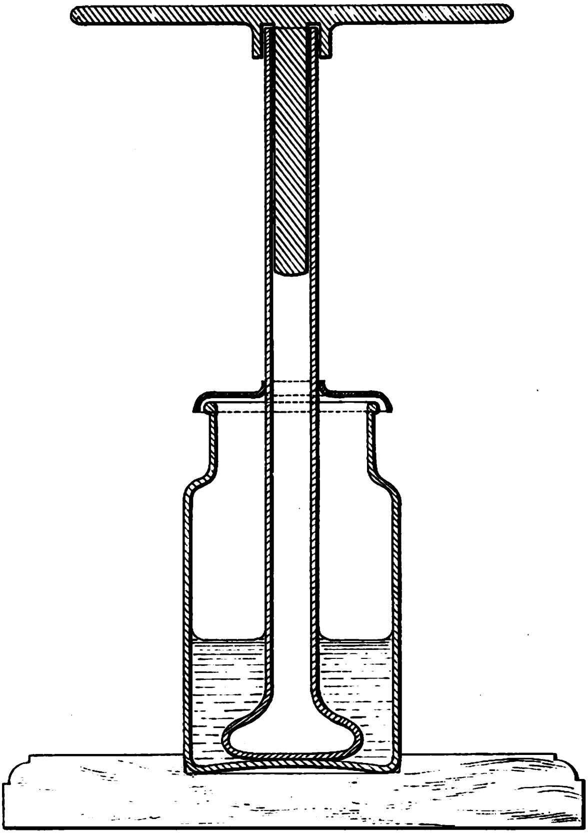


Fig. 1.

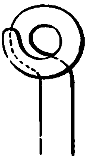


Fig. 2.

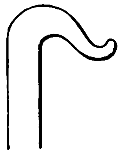


Fig. 3.

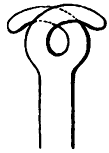


Fig. 4.

Fig. 5.

Scale $\frac{1}{2}$.

Mintern Bros. lith.

Thomson in having the central support of glass solidly fused to the bottom of the jar which holds the acid, and in having the jar formed with a narrow neck instead of a wide one. This construction, though convenient in point of portability and solidity, renders necessary the addition of a tubulure at the side of the vessel by which to introduce the acid. The cost of the apparatus, which cannot be made except by a professional glass-blower, is consequently considerable.

Such supports are, in spite of their cost, of so great utility, especially in a humid climate, that a cheaper substitute of equally high insulation is a desideratum. In the electrical laboratory of University College, Bristol, insulating supports of the type about to be described are found of very great use.

A piece of combustion-tube, about 20 centim. long and 1·4 centim. diameter, is fused together at one end, and the closed end is slightly enlarged by blowing, and then slightly flattened at its extremity. This tube, which forms the central support, is placed upright in a wide-mouthed bottle of hard white flint-glass about 10 centim. high and 5·5 centim. diameter, in which about 50 to 70 grammes of paraffin-wax have been melted. When the paraffin solidifies it contracts greatly; but if the bottle be not too large, holds the stem firmly in its place. To keep out the dust a lid, formed from a disk of gutta-percha about 2 millim. thick, softened by dipping into boiling water, is placed loose-tight on the stem. The arrangement is shown in fig. 1. The upper end of the central tube is open, and affords a convenient means of placing on the support various different objects, such as a metal ball fixed on a metal rod, or a flat metal plate on which to stand any object that is to be insulated. For carrying wires over a table in experiments requiring high insulation, rods of flint-glass of 25 to 30 centim. length, curled at the top in the form of a crook or other hook-form, as shown in figs. 2 to 5, are placed in the central tube of the insulating support, and the wires are slung in them. The insulation-resistance of these supports is many hundreds of megohms even in damp weather. Should a film of dust accumulate on the surface of the paraffin in consequence of any neglect in lowering the gutta-percha cap, the insulator only requires to be warmed to the melting-point of paraffin to restore the lost insulation. For work requiring very special

insulation, sulphuric acid is poured over the top of the paraffin; and in some cases solid stems of glass have been used instead of glass tube. Hitherto there has been no trouble from yielding in the paraffin, which was feared at the outset as a possible fault. A good hard paraffin has been used; and as the flattened form given to the basal enlargement of the glass tube constitutes in itself a foot to the central support, there is little or no tendency for the paraffin to bend under the weight that may be placed for a few hours on the top of the support. To add stability to the apparatus, the bottle is let into a wooden foot. The total cost of the apparatus is less than one tenth of the price charged for the apparatus of Mascart.

NOTE.

In Mr. Glazebrook's Paper on "Curved Diffraction-Gratings," read 14 April, 1883, he gives a geometrical construction for a diffraction-grating without aberration (pp. 251, 252 of this Volume). This construction had previously been given to the Society by Professor Rowland as part of a verbal communication made on 11 Nov. 1882; but Mr. Glazebrook, not having been present on that occasion, was not aware that he had been anticipated on this point. No manuscript of Professor Rowland's communication was sent to the Society, and so the resemblance between his and Mr. Glazebrook's communications in this particular matter escaped notice, until pointed out by Professor Rowland, after Mr. Glazebrook's paper had been published.

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B.

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PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1881-82.

February 12th, 1881.

Prof. W. G. Adams, M.A., F.R.S., Vice-President, in the Chair.

Annual General Meeting.

The Report of the Council was read.
The Treasurer made his financial statement.
The Officers for the Session were elected.

Special General Meeting.

Resolution passed respecting the investment of the Society's funds.

Ordinary Meeting.

The following communications were made:—

"On the Density of Fluid Bismuth." By Prof. W. Chandler Roberts and Mr. T. Wrightson.

"Hydro-mechanical Illustrations of Electrical Phenomena." By Dr. O. J. Lodge.

February 26th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

Special General Meeting.

Confirmation of Resolution passed at Special General Meeting held on February 12th.

Ordinary Meeting.

The following communications were made:—

"Hydro-mechanical Illustrations of Electrical Phenomena" (*continued*). By Dr. O. J. LODGE.

"On the Telegraphic Transmission of Pictures of Natural Objects." By Mr. SHELFORD BIDWELL. With experimental illustrations.

"On an Integrating Machine." By Mr. C. VERNON BOYS.

March 12th, 1881.

Sir WILLIAM THOMSON, LL.D., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

COLVILE BROWNE, F.G.S.; JAMES PRESCOTT JOULE, D.C.L., F.R.S.

The following communications were made:—

"On the Absorption-Spectra of Organic Compounds." By Col. FESTING, R.E., and Capt. ABNEY, R.E., F.R.S.

"On the Definition of 'Work.'" By Mr. D. R. BROWNE.

March 26th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

The following was elected a Member of the Society:—

LEWIS WRIGHT.

The following communications were made:—

"Investigations in Electrostatics." By Dr. J. MOSER.

"On the Electrical Resistance of Thin Films, and on a Revision of Newton's Scale of Colours." By Profs. REINOLD and RÜCKER.

April 9th, 1881.

Prof. W. G. ADAMS, M.A., F.R.S., Vice-President, in the Chair.

The following was elected a Member of the Society:—

Dr. J. MOSER.

The following papers were read:—

"On Thermal Electrolysis." By Dr. J. H. GLADSTONE and Mr. A. TRIBE.

"On Radiation through Ebonite." By Col. FESTING and Capt. ABNEY.

"On Stereoscopic Vision." By Prof. HELMHOLTZ.

May 14th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

The following were elected Members of the Society:—

Mr. D. J. BLAIKLEY; Mr. WALTER KELNER, B.A., M.B.

The following papers were read:—

"On Electric Absorption in Crystals." By Prof. H. A. ROWLAND (Baltimore) and Mr. E. H. NICHOLS.

"On an Absolute Sine Electrometer." By Prof. MINCHIN.

"On the Ascent of Hollow Glass Bulbs through Liquids." By Dr. E. J. MILLS.

May 28th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

The following communications were made:—

"On certain Models to illustrate Fresnel's Theory of Plane Waves." By Mr. C. J. WOODWARD.

"Experiments for the Determination of the Velocity of Light." By Prof. G. FORBES.

"On the Influence of a Powerful Magnet on Strips of Metal through which a Current is flowing." By Dr. E. H. HALL.

June 11th, 1881.

LORD RAYLEIGH, F.R.S., Vice-President, in the Chair.

The following were elected Members of the Society:—

J. E. STEAD; J. E. H. GORDON, M.A.

The following papers were read:—

"On Standard Resistance-Coils." By Prof. J. A. FLEMING.

"On the Hardening and Tempering of Steel." By Prof. W. C. ROBERTS.

"On Curves of Electromagnetic Induction." By W. GRANT.

"On the Opacity of Tourmaline Crystals." By Prof. S. P. THOMPSON.

June 26th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

The following was elected a Member of the Society:—

Señor OLYMPIO DE BARCELOS.

The following communications were made:—

"On Induction Apparatus suitable for Lecture purposes." By W. GRANT.

"On the Index of Refraction of Ebonite." By Profs. AYRTON and PERRY.

"On the Microphonic Action of Selenium-cells." By Dr. J. MOSER.

"On a Standard Cell for Electromotive Force." By Dr. J. MOSER.

"On the Repulsion of a Magnet by a moving Conductor." By Dr. F. GUTHRIE.

"Results obtained with a new Mercury Calorimeter." By Prof. BALFOUR STEWART and Mr. W. STROUD.

"On the Electromotive Force between Liquids and Metals." By Mr. SUTHERLAND.

November 12th, 1881.

Prof. FULLER, M.A., Vice-President, in the Chair.

The following was elected a Member of the Society:—

W. D NIVEN, M.A.

The following communications were made:—

"On Spirals in Crystals." By Mr. LEWIS WRIGHT.

"On a Mode of constructing Water-Pipes so as to prevent their Bursting in Frosty Weather." By Mr. C. VERNON BOYS.

November 26th, 1881.

Prof. W. G. ADAMS, M.A., F.R.S., Vice-President, in the Chair.

Mr. C. VERNON BOYS gave an account of his new Integrating-Machines.

December 10th, 1881.

Prof. W. G. ADAMS, M.A., F.R.S., Vice-President, in the Chair.

The following were elected Members of the Society:—

Lieut. CHARLES E. GLADSTONE, R.N.; ARTHUR W. CLAYDEN, M.A.; WALTER GEORGE WOOLLCOMBE, M.A.; Lieut. CHARLES GAUNTLETT DICKEN, R.N.; Rev. Prof. SEBASTIAN SIRCOM, M.A.

The Members visited the scientific appliances at the Smoke Abatement Exhibition, under the guidance of Prof. W. CHANDLER ROBERTS.

January 28th, 1882.

Dr. W. H. STONE in the Chair.

The following was elected a Member of the Society:—

W. LANT CARPENTER, B.A., B.Sc.

The following papers were read:—

"On the Fluid Density of Metals." By Prof. W. CHANDLER ROBERTS and C. WRIGHTSON.

"On Apparatus for measuring Efficiency." By C. VERNON BOYS.

"On an Electric Meter." By T. VERNON BOYS.

"On the Violet Phosphorescence in Calcium Sulphide." By Capt. ABNEY, R.E.

Annual General Meeting.

February 11th, 1882.

Prof. W. G. ADAMS, M.A., F.R.S., Vice-President, in the Chair.

The following Report of the Council was read by the Vice-President:—

Our Society now enters on the tenth year of its history; for its formation may be considered to have begun with the meeting of the Provisional Committee that followed the issue of a circular addressed by Prof. Guthrie to the leading Physicists of this country in 1873.

The success which attended its formation has been uninterrupted and progressive; and the Society now consists of 331 Members, 14 of whom were elected during the past Session.

Prof. Helmholtz was present at our Meeting in April last, and made a communication to the Society on Stereoscopic Vision.

Of the numerous papers read during the past Session, the following may be specially mentioned:—The first, a communication from America, was a paper read on May 14th, "On Electric Absorption in Crystals," by Prof. Rowland (of Baltimore) and Mr. E. H. Nichols. On the 28th of the same month, Mr. Hall (of Johns Hopkins University) read a paper "On the Influence of a Powerful Magnet on Strips of Metal through which a Current is passing," and described his investigations, which our President subsequently characterized as the most important of the year. The Society will have listened with much pleasure to the valuable series of papers by Mr. C. V. Boys on Mechanical Appliances for Integration.

The most important event of interest to the Society during the past year was the opening of the Electrical Exhibition at Paris in the autumn, and the assembling of the Congress of Electricians, at which our Society was represented by Sir William Thomson, Prof. W. Grylls Adams, and Prof. G. Carey Foster.

After an exhaustive discussion of the question of Electrical Standards, the Congress adopted the definition of electrical units recommended by the British Association, using mercury as a standard. It was, however, considered essential that fresh experiments should be made in order to ascertain the length at zero of the mercurial column, to be adopted in practice as giving a resistance of one ohm with a sufficient degree of accuracy.

The value of the work of the Congress generally was well summed up by M. Mascart, who said (in an official report addressed to the

Minister of Posts and Telegraphs):—"The International Congress, using the Exhibition as if it were a vast laboratory, has secured the greatest possible publicity for the discoveries of scientific men, and has imparted new life to the genius of inventors. Apart from its influence on technical science, the Congress has done good work that must aid the general progress of mankind, at the same time that it serves to fix an important date in the history of Electricity."

It is hoped that the forthcoming Exhibition in this country may prove of equal interest and importance.

During the past year public attention has also been directed to the abatement of smoke; and an Exhibition, now drawing to a close, has been held in the adjacent galleries belonging to the Horticultural Society. The Committee includes several members of our Society; and the work they are endeavouring to carry out is a continuation of that which occupied the attention of Parliament in the Session 1819–20, when a Select Committee was appointed to consider the effect of factory furnaces on public health, and in 1843, when another Committee inquired into the means and expediency of preventing the nuisance of smoke. As this question of the abatement of smoke is of national importance, and as the principles on which perfect combustion depends are as much within the province of Physics as of Chemistry, the Council feel that the subject is well worthy the attention of our Members.

The Committee for Re-publication have not yet been able to commence the reprint of Dr. Joule's scientific papers, of which mention was made in last year's Report. The distinguished physicist, whom we are honoured by counting among our members, has himself kindly undertaken the preparation of the copy for this work, and he has wished to get the greater part of the material ready before actually beginning to print. Dr. Joule promises to supply the first part of the copy in a very short time, in which case the printing will be at once commenced.

The prospect of a speedy beginning of this important undertaking, which will fully engage the Society's resources, has decided the Committee not for the present to begin the reprint of Volta's papers.

With regard to the important question of the protection of buildings from lightning, it will be remembered that the Society appointed Profs. Adams and Foster to represent the Society at a Conference on Lightning-Rods. This Conference has held meetings during the last three years; and after a thorough investigation of the subject, including the work done in this country and

abroad, has issued a comprehensive Report to which evidence of various experts is appended. Copies of this Report may be had by Members of this Society at a reduced price.

The Society has to regret the loss of two of its Members—Dr. Draper and Mr. Louis Schwendler*.

John William Draper, M.D., LL.D., one of our distinguished Honorary Members, President of the Medical and Scientific Faculties of the University of New York, who died January 4, 1882, was an Englishman, having been born at St. Helens, near Liverpool, on May 5, 1811; he was therefore in his seventy-first year.

Up to the age of twenty-two he was resident in his native country, receiving his education, first under private tutors, and afterwards studied Chemistry in the University College, London, then known as the University of London. In 1832 he emigrated to the United States, and continued his studies at the University of Pennsylvania, where (in 1836) he took the degree of M.D. Meantime his talent for original research had manifested itself in the production of several memoirs, which appeared in the 'Journal' of the Franklin Institution. The first of these (published in 1834) was "On the Nature of Capillary Attraction;" while a second was devoted to a discussion of the most eligible method of constructing galvanic batteries of four elements. In 1835 he published an account of some experiments made to ascertain whether light exhibits any magnetic actions. Several branches of the science of electricity subsequently claimed his attention. In 1839 he wrote a memoir, which afterwards was reprinted in the 'Philosophical Magazine,' "On the Use of a Secondary Wire as a Measure of the Relative Tension of Electric Currents." It is instructive to observe in this memoir how Draper's exact mind revolted against the misuse, by writers on electricity, of the words "tension" and "intensity;" and though he himself employed both words, he carefully distinguished between them, using "tension" for what we now call "electromotive force," and "intensity" for the "strength of the current," agreeing, therefore, with the practice of many continental authorities. He also made experiments upon electro-capillary motions, and contributed to the science of thermo-electricity a valuable series of determinations of the thermo-electromotive force of different pairs of metals at different temperatures. In 1837 began the notable

* The following biographical sketches are mainly borrowed from 'Nature' and the 'Telegraph Journal' respectively.

series of researches upon the nature of rays of light in the spectrum, with which the name of Draper will always be associated. His paper that year bore the title, "Experiments on Solar Light;" but it failed to attract much attention in Europe. He was now devoting himself to photography and photo-chemistry with great zeal. His paper "On the Discovery of Latent Light," in 1842, dealt with the images produced by rays of light which are only subsequently developed by some chemical reaction—a process with which the art of photography has made us familiar, but which was then a curious and novel phenomenon. It was Draper who first discovered that in the ultra-violet part of the spectrum there are absorption-bands like the Fraunhofer lines in the visible part of the spectrum. To enumerate the works which proceeded from Draper's pen upon the chemical and physical properties of the ultra-violet, or, as he styled them, "tithyonic" rays would be inadmissible here. Suffice it to say that the greater part of the fifty memoirs mentioned in the Royal Society's 'Catalogue' related to this subject, and the most important of them are to be found reprinted in his 'Scientific Memoirs,' published in 1878. In this volume may be found the pregnant suggestion for a standard of white light for photometry of a piece of platinum-foil, of given size and thickness, raised to a white heat by an electric current of specified strength. To guard against fusion he suggested that an automatic short-circuiting apparatus should be constructed by some "skilled artificer." He thus exactly anticipated Edison's first incandescent lamps, though the satisfactory standard of white light appears to be as far off as ever.

The latest papers Draper published were entitled "Researches in Actino-Chemistry," and treated of the distribution of heat and of chemical force in the spectrum. They appeared in 1872 in the 'American Journal of Science' and in the 'Philosophical Magazine.' During these years of work Draper held important appointments, first in the Hampden-Sidney College, Virginia, where he was Professor of Chemistry, Natural Philosophy, and Physiology; and afterwards (1839) in the University of New York, where he was Professor of Chemistry and Natural History—a post modified two years later into that of Professor of Chemistry in the Medical College of the University. In addition to the original memoirs enumerated above, Dr. Draper wrote several valued text-books of science—a text-book of Chemistry in 1846, and a Human Physiology in 1856, both of which works went through several editions.

Dr. Draper's literary activity manifested itself, however, in other

directions; and he has left an enduring mark in literature as a philosophical historian of no mean merit. The 'History of the Intellectual Development of Europe,' published in 1862, has been translated into all the current languages of European nations. His 'History of the American Civil War,' a work which appeared between the years of 1867 and 1870, when the bitter animosities of the strife were still raging, is distinguished by an impartiality of tone and a philosophical elevation remarkable in a historian, and trebly remarkable in one who wrote in times so little remote from the stirring events recorded. In 1874 Dr. Draper published a 'History of the Conflict between Science and Religion,' a work which attracted some notice, and for which a preface was written by Prof. Tyndall, to introduce the work to English readers. Though unequal to the preceding works in merit, and marred by assumptions that detract from its value, it nevertheless showed great vigour of intellect and philosophical power.

Carl Louis Schwendler.—On the 6th of January, at the village of Schoeneberg, near Berlin, after a long and painful illness, died, at the comparatively early age of forty-three, Carl Louis Schwendler, whose name will long be remembered in connexion with Indian telegraphy. He was born on the 18th of May, 1838, at Torgau, in Prussia, where his father was a captain in an infantry regiment.

Schwendler entered the Berlin Technical Academy as a student in 1856; and had then full opportunity to follow his inclinations, and to combine his technical studies with the course of the Berlin University; and thus he had some of the best teachers—Dove, Grasshoff, Rammelsberg, Weierstrass, Wiehe, Ritter. Shortly after, he happened to come in contact with Dr. Werner Siemens, who at once recognized his talent.

He was in May 1861 engaged upon experiments with the Malta-Alexandria cable. He was Chief Electrician during the manufacture and laying of several cables for the French Government, and in 1863–64 was sent to the Mediterranean to lay one of those cables.

In 1865 he, in conjunction with Mr. Sabine, contributed a short paper to the British Association meeting. The following year, in the 'Philosophical Magazine,' Schwendler published a valuable paper, "On the most suitable Galvanometer-Resistance to be employed in Testing with the Wheatstone Bridge," which subject, till then greatly neglected, he had taken great pains to investigate, both experimentally and mathematically. He retained his appointment with Siemens Brothers until 1868, when Colonel Robinson, the

Director-General of Telegraphs in India at that time, found it was necessary for him to have the aid of an experienced electrician, in order to put uniformity into the system of Government telegraphs there. Schwendler then became Assistant Electrician to the Director-General of Telegraphs in India early in 1868, and remained in that position until the commencement of 1870. Highly appreciated for his integrity, ability, and amiability by the whole staff, he was offered by his chief a commanding position in the Government service as Chief Instructor of Indian Telegraphs; and, acting under official instructions, he, soon after his arrival in India, commenced the preparation of his now well-known 'Testing Instructions,' for the guidance of the Staff of the Government Telegraph Department, the object in view being to facilitate the introduction and thorough understanding on the part of the officials of a rational system of testing.

Besides the performance of his official duties, Schwendler was busy with many collateral subjects of investigation, on some of which he contributed papers to the Asiatic Society, of which he was a Member of Council, and to the 'Philosophical Magazine.' In 1870 he investigated and published "an Arrangement for the Discharge of long Overland Telegraph-Lines," and "A practical method of locating bad Insulators in Telegraph-Lines." In 1872 he published a paper, "On the best Resistance of the Coils of any Differential Galvanometer," and another paper, "On the General Theory of Duplex Telegraphy," and one "On Earth-Currents;" and in 1876 another paper, "On Duplex Telegraphy."

In 1876 Schwendler received leave of absence, and returned to England to recruit his health. While in England, in 1877, he was elected a Member of the Council of the Society of Telegraph Engineers and of Electricians, to which he gave his assistance until he returned to India.

Early in 1877 Schwendler was requested by the Board of Directors of the East-Indian Railway Company to institute detailed inquiries into the position of electric lighting in England, with a view to the illumination of the Indian railway-stations. The series of experiments which Schwendler then carried out was an elaborate and, as far as his materials went, an exhaustive one, occupying him until November 1878. Having to return to India, he was unable to finish a complete report before his departure; but he prepared a *précis*, which was printed and issued.

In March 1879 Schwendler read a paper before the Asiatic

Society of Calcutta on the economy, practicability, and efficiency of the electric light for certain illuminating purposes, and on the best means of its distribution—the substance of his paper being, of course, based upon his experiments made in London. In the same year he published a paper on a new standard of light; and then, reverting to telegraphy, we find a paper, "On a Method of using a small Fraction of the main Current provided by a Dynamo-electric Machine for Telegraphic Purposes." This idea became a favourite one with Schwendler; and as late as last year he published a paper on "Some Experiments to supply all Lines terminating at the Calcutta Station with Currents tapped from a single Dynamo-electric Machine."

Of Schwendler's most recent life and work in India, we know very little. Some years since, we believe, he suffered from an attack brought on by the heat of the climate, which his natural vigour and robustness enabled him subsequently to disregard, if not absolutely to forget, but which possibly laid the foundation for the fatal illness which necessitated his return to Europe, and to which he at length succumbed.

He joined our Society in 1875.

The Council would again call attention to the obligation under which the Society rests to the Lords of the Committee of Council on Education for the use of the Physical Lecture-Room and Laboratories, so generously placed at its disposal; and they have again also to record their gratitude to Dr. Guthrie for his services as Demonstrator.

The Society then proceeded to the election of Council and Officers for the ensuing year; and the following gentlemen were declared duly elected:—

President.—Prof. R. B. Clifton, M.A., F.R.S.

Vice-President (who has filled the Office of President).—Sir Wm. Thomson, LL.D., F.R.S.

Vice-Presidents.—Prof. G. C. Foster, F.R.S.; Prof. F. Fuller, M.A.; J. Hopkinson, M.A., D.Sc., F.R.S.; Lord Rayleigh, M.A., F.R.S.

Secretaries.—Prof. A. W. Reinold, M.A.; Prof. W. Chandler Roberts, F.R.S.

Treasurer.—Dr. E. Atkinson.

Demonstrator.—Prof. F. Guthrie, Ph.D., F.R.S.

Other Members of Council.—Prof. W. G. Adams, M.A., F.R.S.; Prof. W. E. Ayrton, F.R.S.; Walter Baily, M.A.; Shelford Bidwell, M.A., LL.B.; W. H. M. Christie, M.A., F.R.S.; Prof. J. Fleming, D.Sc.; R. J. Lecky, F.R.A.S.; Hugo Müller, Ph.D., F.R.S.; Prof. Osborne Reynolds, M.A., F.R.S.; Prof. A. W. Rücker, M.A.

The following was elected an Honorary Member :—

Prof. G. Quincke.

After the names of the Council and Officers had been announced from the Chair, votes of thanks were passed:—to the Lords of the Committee of Council on Education; to the President; to Prof. Guthrie for his valuable services to the Society; to the Officers; and to the Auditors.

THE TREASURER IN ACCOUNT WITH THE PHYSICAL SOCIETY, FROM DECEMBER 31ST, 1880, TO DECEMBER 31ST, 1881.

Dr.	£ s. d.	£ s. d.
Balance in Bank		167 13 10
„ due by Treasurer		13 17 6
Cheque not presented		4 0 0
Entrance-Fees	16 0 0	
Subscriptions for 1879	2 0 0	
„ 1880	15 0 0	
„ 1881	125 0 0	
„ 1882	10 0 0	
Life Compositions	60 0 0	
		228 0 0
One year's Dividend on £400 4 per cent. Furness Debenture Stock, less Income Tax, 8*s.*		15 12 0
One year's Dividend on £460 5 per cent. Midland Preference Stock, less Income Tax, 11*s.*		22 9 0
One year's Dividend on £200 Metropolitan Board of Works Stock, at $3\frac{1}{2}$, less Income Tax		6 17 0
Sales from Dec. 1, 1880, to Dec. 1, 1881:—		
Wheatstone	6 15 0	
Proceedings	2 0 0	
	8 15 0	
Commission	0 17 6	
		7 17 6
Due to Treasurer		15 4 0
		£481 10 10

Cr.	£ s. d.	£ s. d
Cheque to Chapman		4 0 0
Subscription repaid		1 0 0
Library:—		
Williams and Norgate	2 19 0	
Bookbinding	8 0 0	
		10 19 0
Reports of Meetings		16 4 6
Chapman:—Attendance and Petty Cash		4 10 2
Purchase of £200 4 per cent. Lancaster Corporation Stock		214 0 0
Stationery Box		2 0 0
Stationery and Stamping		4 0 9
Messrs. Taylor and Francis:—		
Vol. iv. (4 parts), Proceedings	128 16 6	
Postage and addressing	12 11 7	
Members' separate copies	26 17 6	
Miscellaneous printing	15 9 0	
		183 14 7
Petty Cash:—		
Mr. A. W. Reinold	2 0 8	
Mr. Roberts	0 14 0	
Dr. Atkinson	2 2 10	
		4 17 6
Balance in Bank		36 4 4
		£481 10 10

Audited and found correct,

London, February 9th, 1882.

SHELFORD BIDWELL,
EDWARD RIGG, } *Auditors.*

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY.

Assets.	£	s.	d.	Liabilities.	£	s.	d.
Balance in Bank	36	4	4	Subscriptions in advance, for 1882	10	0	0
Subscriptions due for 1881 and previous years	38	0	0	Bill to Taylor and Francis	33	0	7
£400 4 per cent. Debenture Stock Furness Railway at 103	412	0	0	Due to Treasurer	15	4	0
£460 5 per cent. Midland Railway Preference Stock at 123	565	0	0	Balance	1416	19	9
£200 Metropolitan Board of Works Stock	210	0	0				
£200 Lancaster Corporation Stock	214	0	0				
	£1475	4	4		£1475	4	4

We have examined the above Account, and also the Securities at the Bank, and find the same to be correct.

London, February 9th, 1882.

SHELFORD BIDWELL,
EDWARD RIGG, } *Auditors.*

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1882–83.

February 11th, 1882.

Annual General Meeting.

Prof. W. G. ADAMS, F.R.S., Vice-President, in the Chair.

The Report of the Council was read.

Election of President, Officers and Council, and Honorary Members.

Ordinary Meeting.

Prof. R. B. CLIFTON, F.R.S., President, in the Chair.

The following papers were read:—

"On the Relation between the Electromotive Force of a Daniell's Cell and the Chemical Affinities involved in its Action." By Dr. C. R. ALDER WRIGHT.

"On the Influence of the Form of Conductors on Electrical Conduction Resistance." By Dr. G. GORE.

February 25th, 1882.

Prof. G. C. Foster, F.R.S., Vice-President, in the Chair.

The following were elected Members of the Society:—

Lieut. Herbert J. Dockrell, R.N.; Charles Richardson; William Ford Stanley, F.M.S., M.R.I.; General H. Hyde, late R.E.; John Buchanan; Prof. George Francis Fitzgerald, M.A.

The following papers were read:—

"On Faure's Accumulators." By Profs. Ayrton and Perry.

"On a Modified Form of Dispersion-Photometer." By Profs. Ayrton and Perry.

"On the Electric Resistance of Carbon under Pressure." By Prof. Silvanus P. Thompson.

"On the Refractive Index and Specific Inductive Capacity of Transparent Insulating Media." By Dr. J. Hopkinson.

"On Regnault's Determination of the Specific Heat of Steam." By Mr. J. Macfarlane Gray.

March 11th, 1882.

Prof. Fuller, M.A., Vice-President, in the Chair.

The following Members were admitted by the Chairman:—

Lieut. Herbert J. Dockrell, R.N.; Mr. Charles Richardson; Mr. William Ford Stanley.

The following was elected a Member of the Society:—

David Rhys Jones.

The following papers were read:—

"On the Influence of Dust and Sulphurous Acid on the Formation of Fog." By Mr. Newth.

"On the Discharge of Electricity by Heat." By Dr. Guthrie.

March 25th, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following Members were admitted by the President:—

Mr. W. Lant Carpenter; Prof. G. F. Fitzgerald.

The following were elected Members of the Society :—

M. J. Jackson, B.A., B.Sc. ; Lazarus Fletcher, M.A.

The following papers were read :—

"On the Influence of Temperature on the Electrical Resistance of Mixtures of Sulphur and Carbon." By Mr. Shelford Bidwell.

"On the Measurement of Curvature and Refractive Index." By Mr. C. Vernon Boys.

"On the Electromagnetic Effects due to the Motion of the Earth." By Prof. G. F. Fitzgerald.

April 22nd, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following Members were admitted by the President :—

Mr. M. J. Jackson ; Mr. J. Buchanan ; Dr. Eugen Obach.

The following was elected a Member of the Society :—

Dr. Edward Hopkinson.

The following papers were read :—

"On the Evidence of a Flowing Liquid moving by Rolling Contact upon the Interior Surface of a Pipe." By Mr. William Ford Stanley.

"Preliminary Notice on the Magnetic Disturbances of the Present Week." By Mr. G. M. Whipple.

May 6th, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following Member was admitted by the President :—

Lazarus Fletcher, M.A.

The following was elected a Member of the Society :—

William Haslam Heaton, B.A.

The following papers were read :—

"On the Construction of Bennet's Galvanic Cell." By Mr. R. J. Lecky.

"On the Mercurial Thermometer." By Mr. F. D. Brown.

"On an Experiment showing the Repulsion between a Moving Conductor and a Magnet." By Dr. F. Guthrie.

May 20th, 1882.

Prof. Fuller, M.A., Vice-President, in the Chair.

The following papers were read:—

"On the Behaviour of Powders under Pressure." By Prof. W. Chandler Roberts.

"On the Mathematical Explanation of Dr. Guthrie's Experiment showing the Repulsion between a Magnet and a Moving Conductor." By Mr. Walter Baily.

"On an Improved Current Meter." By Mr. C. Vernon Boys.

June 10th, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

Major-General C. N. Martin, R.E.

The following papers were read:—

"Experiments on the Vibrations of Tuning-Forks." By Mr. William Ford Stanley.

"On an Integrating Anemometer." By Mr. Walter Baily.

June 17th, 1882.

Meeting in the Clarendon Laboratory, Oxford.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

Prof. W. J. Lewis, M.A. Cambridge.

The following papers were read:—

"On a Simple Electrodynamometer." By Dr. W. H. Stone.

"On a Magnetic Lever for Acting on the Keys of an Organ at a Distance." By Mr. R. H. M. Bosanquet.

"On the Action of an Electrometer-Key." By the President.

"On a Reflecting Galvanometer for Lecture-purposes, which can be used either as a Sine or Tangent Galvanometer." By the President.

"On Reversing and Contact Keys for Galvanic Currents." By the President.

"On a Simple Form of Optical Bench for Measuring the Focal Length and Curvature of Lenses and Mirrors." By the President.

June 24th, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following Members were admitted by the President:—

Mr. Rhys Jones; Major-General C. N. Martin; Mr. Coutts Trotter; Dr. E. Hopkinson.

The following were elected Members of the Society:—

Prof. Bartholomew Price, M.A., F.R.S.; Prof. John Viriamu Jones, M.A.

The following papers were read:—

"Experiments on Vibration." By Prof. Bjerknes, of Christiania.

"On the Determination of Chemical Affinity in terms of Electromotive Force."—Part VI. By Dr. C. R. Alder Wright.

November 11th, 1882.

Prof. R. B. Clifton, F.R.S., President, in the Chair.

The following Members were admitted into the Society by the President:—

Prof. J. C. Adams; Prof. H. A. Rowland.

The following papers were read:—

"On the Theory and Construction of Curved Optical Gratings." By Prof. H. A. Rowland.

"On the Conservation of Energy and the Theory of Central Forces." By W. R. Browne, M.A.

"Historical Notes on Physics." By Prof. S. P. THOMPSON.

(1) On the Original Production of the Electric Light.

(2) On the Boiling of Water under Reduced Pressure.

(3) On the Early History of the Telephone.

November 25th, 1882.

Prof. R. B. CLIFTON, President, in the Chair.

The following papers were read:—

"On Rainbows formed by Light reflected before entering the Rain-drops." By Mr. W. ACKROYD.

"On the Electrical Resistance of Selenium-cells." By Mr. SHELFORD BIDWELL.

"On a General Method of Strengthening Telephonic Currents." By Dr. J. MOSER.

December 9th, 1882.

Prof. R. B. CLIFTON, President, in the Chair.

The following were elected Members of the Society:—

HUGH ERAT HARRISON, B.Sc.; S. T. SAUNDERS, M.A.

The following papers were read:—

"On the Velocity of Light of Different Colours." By Prof. G. FORBES.

"On the Relations between the Distance apart of the Carbons in a Voltaic Arc, the Strength of the Current producing it, and the Opposition to the Current due to the Arc." By Profs. AYRTON and PERRY.

"On the Intensities of the Magnetic Field due to Electromagnets in which the Distribution of the Wire was Variable." By Profs. AYRTON and PERRY.

Exhibition of twenty Swan Lamps, rendered incandescent by three Faure Accumulators.

January 27th, 1883.

Prof. R. B. CLIFTON, President, in the Chair.

The following Member was admitted by the President:—

Mr. H. E. HARRISON.

The following papers were read:—

"On the Methods which have been employed for Measuring Electrical Resistance in Absolute Units, with details of a Method proposed by himself." By Prof. G. CAREY FOSTER.

"On the Locus of the Spectra formed by Curved Gratings." By Mr. WALTER BAILY.

Annual General Meeting.

February 10th, 1883.

Prof. W. G. ADAMS, M.A., F.R.S., Vice-President, in the Chair.

The following Report of the Council was read by the Vice-President:—

A retrospect of the proceedings of the Society during each succeeding year has been given in the successive Annual Reports.

In the present year a more comprehensive view may fairly be taken, as this Annual Meeting completes the first decade of our history. It is true that the first Meeting of the Society was not held until the 21st of March 1874, and, further, that the Society can only date its legal existence from 1876, the year in which it was formally registered in accordance with the provisions of the 23rd Sect. of the Public Companies Act 1862; but its formation as a Society may be considered to have begun with the meeting of its Provisional Committee in 1873.

Its history has been that of Physical Science generally in the United Kingdom for the past ten years, for almost every physicist of eminence has either joined our ranks or has communicated a paper to our Meetings.

We have established an enduring bond of union with our fellow workers abroad; and the list of our Foreign Honorary Members,

which the Council will probably seek powers to extend, is sufficient to indicate the comprehensive and representative character of our organization.

The Council had hoped to be able to report that death had dealt gently with the Members of our Society, and that during the past year no one had dropped from our ranks; but yesterday one of our most distinguished Members, who only a few days ago was employing his extraordinary intellectual powers for the advancement of various branches of science, passed from among us, and we, in common with many other Societies, have to mourn an irreparable loss.

Henry John Stephen Smith, Savilian Professor of Geometry in the University of Oxford, died at Oxford on the 9th of February, at the age of fifty-six.

Ever since his admission to the University as Scholar of Balliol College, the remarkable powers of Henry Smith, both in literature and science, have been conspicuous. His Undergraduate career, brilliant almost without a parallel, was followed by a life of unceasing activity, in which his marvellously varied attainments, combined with his great capacity for business and his unflagging energy, excited the admiration of all whose good fortune it was to work with him; while his genial manners and his many social gifts endeared him to a constantly increasing circle of friends, from which, it may safely be said, no one who was once admitted ever retired.

Henry Smith was born in Dublin; but he has lived in England since the death of his father, which occurred when he was only two years old; and he was educated first at home and afterwards at Rugby.

In 1848 he gained the Ireland Scholarship founded "for the promotion of Classical learning and taste." In 1849 he passed the examination for the degree of B.A., being placed in the first class both "in Literis Humanioribus" and "in Disciplinis Mathematicis et Physicis." In 1851 he won the Senior Mathematical Scholarship; and he had thus obtained the highest distinctions which the University of Oxford has to confer both for Classics and Mathematics. He speedily became Fellow of Balliol College, and undertook the office of Mathematical Tutor in his College; an office which he retained until a few years ago, when he exchanged his Fellowship in Balliol for one in Corpus Christi College.

In 1861 he was elected to the Savilian Professorship of Geometry

rendered vacant by the death of Professor Baden Powell; and in 1874 he succeeded Professor Phillips as Keeper of the University Museum. Both these appointments he held up to the time of his death.

For many years he was a Member of the Hebdomadal Council of the University, and took a prominent part in the management of University business; while at the same time the citizens of Oxford always found him ready to assist in any movement for the promotion of their welfare or for the advancement of their educational institutions.

Lavishly as Professor Smith spent his time in the service of the University and city, his energy enabled him to discharge other most important and responsible duties. He was a Member of the Royal Commission on Scientific Education, and recently of the Commission, appointed under the Universities of Oxford and Cambridge Act 1877, charged with the task of making new statutes for the University and Colleges of Oxford. By these statutes the relations between the University and the Colleges have been considerably modified, and an attempt has been made to bring the constitution of both more into harmony with the requirements of the time; but he only lived to see the commencement of this great change; and the loss of his calm judgment and his guiding hand during the transition period makes his early death the more to be deplored.

Professor Smith was an active Member of the Royal Society, of the Royal Astronomical Society, of the London Mathematical Society (of which he was recently the President), and of the British Association for the Advancement of Science; he was also chosen in 1880 as a Corresponding Member of the Academy of Sciences in Berlin.

When the Government decided to place under the control of a Special Council the management of the Meteorological Office, and the organization of arrangements for collecting and utilizing meteorological observations, Professor Smith was selected as Chairman of this Council; and he carried on this onerous and responsible work to the last, in spite of an accumulation of other labours which would have completely overwhelmed most men, even if endowed with unusual powers of endurance.

It might seem that in the discharge of the duties already noticed the time of a busy life would be completely accounted for; but the principal work of Professor Smith has been scarcely mentioned. Before and above all he was a Mathematician. In Mathematics centred his strongest interests, to this study he devoted whatever

time he could snatch from his other occupations, and by his investigations in various branches of this subject he will be known to posterity, when the remembrance of his other labours has faded away.

His attention was specially directed to Pure Geometry, to the Theory of Elliptic Functions, and to the Theory of Numbers. On the first of these subjects he published little; but his admirable lectures have instructed many successive generations of Oxford students, and have had a marked influence on the study of Geometry in that University. Sad as it is that he was prevented from carrying out his intention of arranging these lectures for publication, it is to be hoped that they may not be entirely lost; his own notes, combined with those taken by his pupils, may yet furnish materials for a most valuable work.

On the Theory of Elliptic Functions and allied subjects several papers, published for the most part in the Transactions of English and foreign scientific Societies, bear ample testimony to Professor Smith's great power of dealing with different and refined analytical problems, and suffice by themselves to place him in the first rank of contemporary Mathematicians. But probably his fame will rest mainly upon his contributions to his favourite subject—the Theory of Numbers. His Reports (unfortunately incomplete) to the British Association on the Theory of Numbers, though professedly only an account of knowledge already acquired, are full of original matter, and the manner in which he has connected the work of his predecessors by his own methods will be an enduring memorial of his extensive knowledge, his mathematical genius, and his admirably finished style. These Reports, combined with communications to the Royal Society, the French Academy, and other Societies, will assuredly secure for Professor Smith a most prominent place among the writers on the Theory of Numbers, and cause him to be associated with Gauss, Lejeune-Dirichlet, Jacobi, Legendre, and Cauchy.

Early in his career Professor Smith gave some attention to Experimental Science, and, although other studies drew him away from active work in the domain of Physics, he always took the greatest interest in the progress of that subject, and he used his powerful influence in promoting the study of it in Oxford. To his ever ready cooperation and his wise counsel whatever success has attended the development of the department of Physics in the University may be largely ascribed.

The study of Mathematics is so absorbing to those engaged in extending its boundaries, that they are frequently led to seek retire-

ment and to stand aloof from the movements taking place in the society in which they live. This, however, was not the case with Professor Smith; for him all social and political questions had the greatest attraction, and, when in 1878 a vacancy occurred in the representation of the University in Parliament, he allowed himself to be nominated as successor to Mr. Gathorne Hardy, who had been raised to the Peerage. His supporters, however, though neither few nor undistinguished, formed but a forlorn hope: the struggle ended in their complete defeat; and thus the University of Oxford threw away the chance of being represented by the most gifted of her sons, and the country lost the opportunity of profiting by his keen intellect, his wide knowledge, and his persuasive eloquence.

The Society now consists of 340 Members, 19 of whom were elected during the present year. During the past ten years about 330 communications have been made to our Meetings, the distinctive feature of which has been the abundant and elaborate experimental illustrations that have, in nearly all cases, accompanied the reading of the papers. This we mainly owe to the care of our Demonstrator, Dr. Guthrie, whose watchfulness over our interests has been unceasing since the time his efforts resulted in our formation as a Society.

Our Financial affairs, so ably managed by Dr. Atkinson, who has been Treasurer since the formation of our Society, are in a thoroughly satisfactory condition; and the Accounts show that we possess neither the poverty that would cripple our usefulness, nor riches which might tend to make us rely less than has hitherto been the case on the individual efforts of our Members.

The keynote struck by Mr. Fleming in the new Contact Theory of the Galvanic Cell, the first paper read to our Society, has been well maintained, as no less than one third of our papers during the past ten years have been on subjects connected with electricity.

The determination of the Ohm in relation to the British-Association Unit has been actively carried on by several of our Members—Prof. G. C. Foster, Lord Rayleigh, Mr. Glazebrook, and Professor Rowland. It will be remembered that the Electrical Congress that met in Paris in 1881 decided to retain the Mercury Standard for reproduction and comparison, and, while adopting the absolute system of the British Association, referred the final determination of the unit measure of resistance to an International Committee which met in Paris in 1882.

The more remarkable contributions to the Society during the past year consisted of discourses by Prof. Bjerknes, of Christiania, and Prof. Rowland, of Baltimore. Prof. Bjerknes (who has devoted twenty-five years to tracing analogies between hydrodynamical phenomena and those of electricity and magnetism) illustrated the static attraction and repulsion of electricity and magnetism, as well as electrodynamic attractions and repulsions. The Members will remember that he employed small tambours, pulsating near each other in water or other media, to which the vibrations were imparted; the more novel effects consisting in representations of the mutual action of two electric currents flowing in the same direction. The experiments were of remarkable beauty.

Prof. Rowland exhibited on November 17 a number of his new and beautiful concave gratings, which consist of slabs of speculum metal ruled with lines varying from 5000 to 42,000 to the inch; one result of Prof. Rowland's investigation with these instruments being the division of certain lines of the spectrum that have not hitherto been separated.

Mr. W. Chandler Roberts, finding that his official duties render it impossible for him to devote the necessary time to the service of our Society, retires from the Secretaryship he has held since 1874. The announcement of his retirement has been received with general regret; but the Council felt that, considering Prof. Roberts's recently increased engagements, they could not fairly urge him to retain his post.

They are fortunate in having secured as his successor Mr. Walter Baily, who is already favourably known to our Members by his communications to our Proceedings.

It has been suggested that notices of the titles of papers to be read at our Meetings should be sent to each Member of the Society; and the Council, anxious at all times to carry out the expressed wishes of the Members, has arranged that all who desire to have such notices shall receive them gratuitously on application to Messrs. Taylor and Francis at the beginning of each Session.

With regard to the publications of the Society other than the 'Proceedings,' the Council have to report that the works of Dr. Joule are in print with the exception of about 100 pages, which the author has promised to furnish very shortly; and the Council confidently expect that the volume will be issued early in the present year. The translation and reprinting of Volta's works will remain in abeyance until the completion of Dr. Joule's volume.

The Council has also under consideration the publication of a Memoir of Dr. Gilbert, Physician to Queen Elizabeth. The accuracy and importance of his chemical and physical work has not hitherto been generally recognized; and as the Memoir will be written by Prof. Ferguson, of Glasgow, whose eminence as an historian of science is well known, it cannot fail to be of interest and value.

The Librarian reports that the Library continues to be in a very satisfactory condition; and the Council will shortly be asked to vote the sum necessary to defray the cost of binding the periodicals. Further efforts would, however, appear to be needed in order to effect an interchange of our publications with those of other Societies and Institutions.

The Society then proceeded to the election of Officers and Council for the ensuing year; and the following were declared duly elected:—

President.—Prof. R. B. Clifton, M.A., F.R.S.

Vice-President (who has filled the Office of President).—Sir Wm. Thomson, LL.D., F.R.S.

Vice-Presidents.—Prof. G. C. Foster, F.R.S.; J. Hopkinson, M.A., D.Sc., F.R.S.; Lord Rayleigh, M.A., F.R.S.; Prof. W. Chandler Roberts, F.R.S.

Secretaries.—Prof. A. W. Reinold, M.A.; Walter Baily, M.A.

Treasurer.—Dr. E. Atkinson.

Demonstrator.—Prof. F. Guthrie, Ph.D., F.R.S.

Other Members of Council.—Prof. W. G. Adams, M.A., F.R.S.; Prof. W. E. Ayrton, F.R.S.; Shelford Bidwell, M.A., LL.B.; W. H. M. Christie, M.A., F.R.S.; Prof. F. Fuller, M.A.; R. T. Glazebrook, M.A., F.R.S.; R. J. Lecky, F.R.A.S.; Prof. O. J. Lodge, D.Sc.; Hugo Müller, Ph.D., F.R.S.; Prof. J. Perry.

The following was elected an Honorary Member:—
Prof. Ludwig Boltzmann.

After the names of the Council and Officers had been announced from the Chair, votes of thanks were passed:—to the Lords of the Committee of Council on Education; to the President; to the other Officers; and to the Auditors.

THE TREASURER IN ACCOUNT WITH THE PHYSICAL SOCIETY, FROM DECEMBER 31ST, 1881, TO DECEMBER 31ST, 1882.

Dr.	£ s. d.	£ s. d.
Balance in Bank		36 4 4
19 Entrance-Fees	19 0 0	
Subscriptions for 1879	3 0 0	
" 1880	10 0 0	
" 1881	17 0 0	
" 1882	121 0 0	
" 1883	2 0 0	
Life Compositions	100 0 0	
		272 0 0
One year's Dividend on £400 Furness 4 per cent. Debenture Stock, less Income Tax	15 13 4	
One year's Dividend on £460 5 per cent. Midland Preference Stock, less Income Tax	22 10 5	
One year's Dividend on £200 3½ per cent. Metropolitan Board of Works Stock, less Income Tax	6 16 10	
One year's Dividend on £200 Lancaster Corporation Stock, less Income Tax	7 16 8	
		52 17 3
Sales to November 30, 1882:—		
Wheatstone	3 18 9	
Proceedings	3 4 0	
	7 2 9	
Commission	0 14 3	
		6 8 6
		£367 10 1

Cr.	£ s. d.	£ s. d.
By Subscription repaid		1 0 0
Balance due to Treasurer, Dec. 31, 1882		15 4 0
Taylor and Francis:—		
Proceedings, vol. iv. (3 parts), vol. v. (parts 1 & 2)	88 12 0	
Postage and addressing	8 17 0	
Members' separate copies	15 18 0	
Miscellaneous printing	16 16 4	
Wheatstone	0 15 10	
		130 19 2
Reports of Meetings		10 0 0
Mertens:—Custom-House expenses for Prof. Bjerknes' Apparatus		5 5 0
Williams and Norgate:—Periodicals		4 0 0
Chapman:—Attendance and Petty Cash		4 6 10
Richardson:—Stationery and Printing		1 10 6
Petty Cash:—		
Mr. Reinold	1 19 8	
Mr. Roberts	0 15 2	
Mr. Atkinson	2 0 4	
		4 15 2
Clarke		0 18 0
Balance in Bank		138 0 9
" hands of Treasurer		51 10 8
		£367 10 1

Audited and found correct,

E. W. JONES,
F. W. BAYLY, } *Auditors.*

London, February 9th, 1883.

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY.

Assets.	£	s.	d.	Liabilities.	£	s.	d
Balance in Bank	138	0	9	Subscriptions in advance, for 1883	2	0	0
„ hands of Treasurer	51	10	8				
Estimated Subscriptions due	35	0	0				
£400 Furness 4 per cent. Debenture Stock	412	0	0	Balance	1623	11	5
£460 Midland 5 per cent. Preference Stock	565	0	0				
£200 Metropolitan Stock	210	0	0				
£214 Lancaster Corporation Stock	214	0	0				
	£1625	11	5		£1625	11	5

We have examined the above Account, and also the Securities at the Bank, and find the same to be correct.

E. W. JONES,
F. W. BAYLY, } *Auditors.*

London, February 9th, 1883.

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1883-84.

February 10th, 1883.

Prof. Fuller, Vice-President, in the Chair.

Annual General Meeting.

The Report of the Council was read.
The Treasurer made his Financial Statement.
The Officers for the Session were elected.

Ordinary Meeting.

The following communication was made :—

"On a Graphic Method of Measuring the Efficiency of an Electric Motor." By Prof. S. P. Thompson.

February 24th, 1883.

Prof. CLIFTON, President, in the Chair.

The following were elected Members of the Society:—

Prof. A. W. SCOTT, M.A.; F. J. M. PAGE, B.Sc.

The following communications were made:—

"On Optical Combinations of Crystalline Films." By LEWIS WRIGHT.

"Experimental Demonstration of the Vortical Theory of the Solar System." By PHILIP BRAHAM.

March 10th, 1883.

Prof. G. C. FOSTER, Vice-President, in the Chair.

The following was elected a Member of the Society:—

Major W. S. BOILEAU, R.E.

The following communications were made:—

"On a Method of Measuring Electrical Resistances with a Constant Current." By SHELFORD BIDWELL.

"On certain Molecular Constants." By Dr. GUTHRIE.

April 14th, 1883.

Prof. G. C. FOSTER, Vice-President, in the Chair.

The following were elected Members of the Society:—

WILLIAM FREDERICK SMITH; GEORGE FORBES, M.A., F.R.S.E.

The following communications were made:—

"On Science Demonstration in Board Schools." By W. LANT CARPENTER.

"On a Polarizing Prism." By R. T. GLAZEBROOK.

"On Curved Diffraction Gratings." By R. T. GLAZEBROOK.

"On the Viscosity of a Solution of Saponine." By W. H. STABLES and A. E. WILSON.

April 23rd, 1883.

Prof. CLIFTON, President, in the Chair.

The following communications were made:—

"On Colour Sensations." By H. R. DROOP.

"On a New Photometer." By Sir JOHN CONROY.

"On the Causes and Consequences of Glacier Motion." By W. R. BROWNE.

"On a New Spectrometer." By the PRESIDENT.

May 12th, 1883.

Prof. CLIFTON, President, in the Chair.

The following was elected a Member of the Society:—

ALFRED WALKER SOWARD.

The following communications were made:—

"On an Experiment illustrating Motion produced by Diffusion." By C. J. WOODWARD.

"Some Uses of a New Projection-Lantern." By W. LANT CARPENTER.

"On the Determination of Chemical Affinity in terms of Electromotive Force.—Part VII. On the E.M.F. of Clark's Mercurous Sulphate Cell." By Dr. C. N. ALDER WRIGHT and C. THOMPSON.

"On the Complete Determination of a Double Convex Lens by Measurements on the Optical Bench." By the PRESIDENT.

May 26th, 1883.

Prof. CLIFTON, President, in the Chair.

The following communications were made:—

"On the Graphical Representation of Musical Intervals." By G. GRIFFITH.

"On a Phenomenon of Molecular Radiation in Incandescent Lamps." By J. A. FLEMING.

"On the Crossing of Rays." By WALTER BAILY.

"On a Modified Form of Electric Insulator." By the PRESIDENT.

June 9th, 1883.

Prof. Clifton, President, in the Chair.

The following communications were made:—

"On an Improved Construction of the Movable Coil Galvanometer for Determining Current and E.M.F. in Absolute Measure." By Dr. E. Obach.

"On the Electric Resistance of Water." By Profs. W. E. Ayrton and J. Perry.

"On Apparatus for Experiments on Centrifugal Force." By Profs. W. E. Ayrton and J. Perry.

"On Measuring the Moment of Inertia of a Fly-wheel." By Profs. W. E. Ayrton and J. Perry.

June 23rd, 1883.

Prof. Clifton, President, in the Chair.

The following was elected a Member of the Society:—

C. H. Stearn.

The following communications were made:—

"On the Cause of Evident Magnetism and Neutrality in Iron and Steel." By Prof. D. E. Hughes.

"On the Sine Electrometer." By Prof. G. Minchin.

"On the Electric Conductivity and other Properties of the Copper-Antimony Alloys." By G. Kamensky.

November 10th, 1883.

Prof. Clifton, President, in the Chair.

The following communications were made:—

"On the Velocity of Sound in Tubes of Various Diameters." By D. J. Blaikley.

"On the Moment of a Compound Magnet." By R. H. M. Bosanquet.

"On the Electrical Resistance of the Skin, and on certain Medico-Electrical Appliances." By W. Lant Carpenter.

November 24th, 1883.

Prof. Clifton, President, in the Chair.

The following communications were made:—

"On the Purification of Mercury by Distillation *in vacuo*. By J. W. Clark.

"On a Method of Determining the Constant of an Electrodynamometer." By A. P. Shattock.

"On the Measurement of the Curvature of Lenses, and the Refractive Indices of Liquids by means of Newton's Rings." By the President.

December 8th, 1883.

Prof. G. C. Foster, Vice-President, in the Chair.

The following were elected Members of the Society:—

Walter G. Gregory, B.A.; Major C. A. Macgregor, R.E.; James Walker, M.A.

The following communications were made:—

"On Dolbear's Static (Condenser) Telephone, and its Use in an Apparatus analogous to Wheatstone's Bridge for Comparing the Capacities of two Condensers." By Prof. S. P. Thompson.

"On a Simple Form of Insulating Stand." By Prof. S. P. Thompson.

"On an Illustration of certain Electrostatic Phenomena by means of Mayer's Floating Magnets." By Prof. S. P. Thompson.

"On Experiments illustrating Attraction and Repulsion. By J. Monckman.

"On an Integrating Anemometer." By Walter Baily.

January 26th, 1884.

Prof. Clifton, President, in the Chair.

Special General Meeting.

Resolutions were passed to make certain alterations in the Articles of Association of the Society, which would give to Articles 36, 39, and 48 the following form:—

Art. 36. "The affairs of the Society shall be managed by a

Council consisting of a President, Permanent Vice-Presidents, not more than four nor less than two other Vice-Presidents, two Secretaries, a Treasurer, a Demonstrator, and not more than ten other persons, all of whom must be Ordinary Members of the Society."

ART. 39. "At each Annual General Meeting all Members of the Council, except the Permanent Vice-Presidents, shall retire from Office, and the Society shall elect a New Council in manner herein provided."

ART. 48. "Every Member of the Society who has filled the Office of President shall be a Permanent Vice-President."

Ordinary Meeting.

The following was elected a Member of the Society:—

FUNG YEE.

The following communications were made:—

"On the Construction and Adjustment of Direct Reading Electric Measuring Instruments." By Profs. W. E. AYRTON and J. PERRY.

"On the Determination of Chemical Affinity in terms of Electromotive Force.—Part VIII. On the E.M.F. set up during Interdiffusion." By Dr. C. R. ALDER WRIGHT and C. SIMPSON.

Annual General Meeting.

February 9th, 1884.

Prof. CLIFTON, President, in the Chair.

The Resolutions passed at the Special Meeting on January the 26th were confirmed.

The following Report of the Council was read by the President:—

The Session of the Society which has just been completed, though not marked by the communication of any great discovery, has been full of steady work. The Meetings have been well attended, and the papers and discussions have fully occupied the time at our disposal. Several interesting communications on lenses have been made by the President, of which the Council hope before long to be

furnished with the MSS., so that these communications may be published in the 'Proceedings.' The beautiful optical effects produced by Mica Films, prepared by Mr. Lewis Wright, and the striking experiments on Magnets, shown by Prof. Hughes, will be vividly remembered. A careful series of measurements on the Velocity of Sound in Air has been made by Mr. Blaikley; and the important subjects of Colour-sensations and Colour-blindness have been brought before the Society by Mr. Droop.

In the report of 1883 it was mentioned that the Council had arranged that all Members who desired notices of the titles of papers to be read at our Meetings should receive them gratuitously on application to Messrs. Taylor and Francis. During the past Session these notices have been sent two or three days before each Meeting to about seventy Members, and it has been possible by this means to announce communications of which the Secretaries have received notice too late for announcement in the weekly papers.

The Council have for some time felt the difficulty of retaining the services of those who have held the important office of President, and at the same time of recommending at each election a sufficient number of new Members of Council. In order to remove this difficulty the Council have proposed, and the Society has made, a change in the Articles of Association, which will give to all past Presidents the position of permanent Vice-Presidents. This is a rule in some other societies, and the Council believe that its adoption will be found advantageous.

The Library has been rearranged. Volumes and numbers required to complete sets have been obtained, either by purchase or gifts. The publications requiring it have been prepared for binding. The whole has been catalogued, and consists of

	Volumes.
Separate Works	480
Transactions of Societies, Proceedings, and Periodicals	200
Separate memoirs	88
	768

The Library has been enriched by several donations, the principal of which is a recent and most acceptable gift, by Lady Siemens, of 119 volumes, forming part of the library of her late husband, whose lamented death has deprived this Society of one of its most honoured members. The Society is also indebted to Dr. Guthrie for gifts of many valuable works.

The printing of the first volume of Joule's works is now finished, and the sheets are ready for the binders. A specimen-copy has been bound for inspection, and the volume will shortly be in the hands of Members. The ready access to these important papers will be a valuable aid to the prosecution of Physical Research. Some progress has been made in the translation of Volta's works, but the death of the gentleman to whom the work was entrusted has caused an interruption, and fresh arrangements will have to be made by the Council.

The Council would again call attention to the obligation under which the Society rests to the Lords of the Committee of Council on Education for the use of the Physical Lecture-room and Laboratories, so generously placed at their disposal.

The Society has to deplore the loss by death of an unusually large number of its Members, including several of the foremost men of science. The last annual report was read immediately after the death of Henry John Stephen Smith. In this report we have to record the deaths of the President of the Royal Society, of Sir William Siemens, of M. Plateau, of Mr. Cromwell Fleetwood Varley, of Lieut. Hastings R. Lees, and of Mr. C. Merrifield.

William Spottiswoode, descended from an old Scotch family which has produced many distinguished members, was born in London on January 11, 1825. He entered Balliol College, Oxford, in 1842, took a first-class in Mathematics in 1845, and obtained the Junior, and afterwards the Senior Mathematical Scholarships. He is described as having already an extraordinary liking for, and great skill in, the Morphology of Mathematics. Upon leaving Oxford he entered into the business of the Queen's Printers, which was resigned to him by his father. He continued in that business throughout his life, and it was largely developed under his care. He was elected a Fellow of the Royal Society in 1853, and held the office of Treasurer from 1871 to 1878. In 1879 he became President, and held that office until his death. He was President of the British Association in 1878, and of the London Mathematical Society from 1870 to 1872. He was admitted a Member of this Society in its first Session in 1874. He received the degree of LL.D. at Cambridge, Dublin, and Edinburgh, and that of D.C.L. at Oxford. Mr. Spottiswoode's greatest distinction was earned in the region of Pure Mathematics, and his numerous and important papers were specially characterized by elegance and symmetry. Up to the year of 1871 his communi-

cations to science appear to have been almost entirely confined to Mathematics; but about that time he began to devote his attention more to Physics, and he was naturally attracted to that branch of Physics in which the elegance and symmetry he delighted in are most conspicuously shown. In 1871 he published in the 'Philosophical Magazine' a paper "On some Experiments on Successive Polarization made by Sir C. Wheatstone." This was followed in 1872 by a paper in the 'Proceedings' of the Royal Society, "On the Rings produced by Crystals when submitted to circularly Polarized Light," and by a paper in 1874, "On Combinations of Colour by Polarized Light" (also communicated to this Society). In 1875 a communication "On a revolving Polariscope" was made to the Royal Institution and to this Society. The other Physical subject to which his attention was specially given was the Electric Discharge *in vacuo*. In this investigation he spared no expense in apparatus, and his well-known induction-coil is the largest which has been made. His papers on this subject, published in the 'Proceedings' of the Royal Society, are:—1874–75, "On Stratified Discharges through Rarefied Gases;" 1876–77, "Observations with a Revolving Mirror," and "On a rapid Contact-breaker, and the Phenomena of the Flow" (also communicated to this Society); 1877, "On Stratified and Unstratified forms of the Jar-Discharge," and "Photographic Image of the Stratified Discharge;" 1878, "Discharge from a Condenser of large Capacity;" 1879, "On the Sensitive State of Electrical Discharges through Rarefied Gases" (also communicated to this Society); 1879–80, "On some of the Effects produced by an Induction-Coil with a de Meritens Magneto-electric Machine," and "On the Sensitive State;" 1881, "On Shadows of Striæ and Multiple Radiations from Negative Terminal," and "On the Movement of Gas in Vacuum Discharges." Several of the latter papers were prepared in combination with Mr. J. F. Moulton. As President of the Royal Society Mr. Spottiswoode was able to bring great capacity for business, unerring tact, and wide-spread sympathy to promote the advancement of science. He died of typhoid fever on June 27, 1883. The high estimation in which he was held, and the deep regret felt for his loss, are shown in the very general request that he should be buried in Westminster Abbey, where he now lies, among those great men whom England desires to hold in special remembrance.

Charles William Siemens was born at Leuthe, in Hanover, on

April 4, 1823. In 1843 he came to England to introduce a new method of electro-gilding, invented by himself and his brother Werner, and was enabled by the help of Mr. Elkington to bring it into practical use. The next year he returned to England with his Chronometric Governor, a modification of which is now in use in the Greenwich Observatory for regulating the motion of instruments. In 1850 he received from the Society of Arts its Gold Medal for his Regenerative Condenser. In 1853 he read, before the Society of Civil Engineers, a paper "On the Conversion of Heat into Mechanical Effect," for which he received the Telford Premium and the Medal of the Institution. In 1857, in conjunction with his brother Frederick, he brought out what was probably his greatest invention, the Regenerative Gas-Furnace, in which the heated products of combustion are made, on leaving the furnace, to give up their heat to the gas and air which are entering. The very high temperature obtained in such a furnace enabled Mr. Siemens to develop a new method of manufacturing steel, which has been so successful that steel made by his method is largely used in the manufacture of boilers and hulls of ships. In 1867 the principle underlying all the dynamo machines was communicated by him to the Royal Society in a paper, "On the Conversion of Dynamical into Electrical Force without the aid of Permanent Magnetism." The invention was due to Werner Siemens; but it had also occurred to Sir Charles Wheatstone, and was announced by him to the Royal Society in a paper read by him on the same evening as that on which Mr. Siemens's paper was read. The firm of Siemens Brothers has been largely employed in the construction of submarine cables, and more recently in that of electrical machines. The Portrush and Bushmill Electric Tramway, completed only two months before his death, forms one of Dr. Siemens's greatest achievements. He made some experiments on the growth of plants under the electric light, and took a great interest in the construction of arrangements for the consumption of smoke. He was made a D.C.L. of Oxford in 1870, and was knighted in March last, besides receiving many other honours. He was elected a Fellow of the Royal Society in 1862, and served on the Council in 1869 and 1870. He was admitted a Member of this Society in its first Session in 1874. Sir William Siemens combined great amiability of character with resistless energy, and clear knowledge of scientific principles with boundless futility of invention. He has been preeminent in the application of science to the wants of life; and his loss must needs make our progress in this

application slower than it might otherwise have been. He died on November 19, 1883.

Lieut. Hastings R. Lees was born on January 4, 1853. He entered the Navy as Naval Cadet on the 'Britannia' in 1866, and attained the rank of Lieutenant in 1875. After passing through the usual courses of study at Greenwich and Portsmouth in order to qualify for a Gunnery-Lieutenant, he was appointed to the 'Swiftsure' flagship in the Pacific. In October last, when on shore at Vancouver Island, he was thrown from a carriage and died from the injuries he then received.

Lieut. Lees was a promising officer, and keenly interested in all scientific matters connected with his profession. He became a Member of this Society in March 1879.

Cromwell Fleetwood Varley was born in the year 1828, and in 1846 he entered the service of the then infant Electric Telegraph Company. He not only had had a good scientific training, but he was well versed in the use of tools; and having a remarkable aptitude for the application of scientific knowledge to practical purposes, he soon made himself a valuable and useful officer. He remained in the service of the Telegraph Company until 1866, having attained the highest office open to him, and having very materially improved the working of their system. From that date he became associated with Atlantic-Cable enterprises, and, in conjunction with Sir William Thomson and Professor Fleeming Jenkin, he solved the difficult problem of long-cable working. His career was connected entirely with telegraphy. He took out no less than thirty-three patents. He introduced accurate measurement in testing lines and localizing faults; but his principal invention was the application of condensers to expedite the working of long submarine cables. The specifications of his numerous patents contain a mass of original observations that anticipated much later work; and he had a remarkable skill in applying the crude ideas of others and seizing upon their practical bearing. As an experimental electrician he was unrivalled, and his skill was remarkably shown in localizing faults at the bottom of the ocean.

Among the more important of Mr. Varley's purely scientific papers may be mentioned two papers in the 'Proceedings' and 'Transactions' of the Royal Society. One of these was a remarkable investigation into the nature of the electric discharge in rarefied

gases, in which he proved that appreciable mechanical force was exerted upon surfaces exposed to the discharge, a fact since more fully investigated by Mr. Crookes. The second related to the influence of electrification on the capillary properties of a surface of mercury in contact with diluted sulphuric acid, a subject to which the subsequent researches of Mons. G. Lippmann have attracted a considerable amount of attention. He died on the 2nd of September last.

Joseph Antoine Ferdinand Plateau was born at Brussels on the 14th of October, 1801. He studied Mathematics and Physics, after leaving school, at the University of Liége, where he received the degree of Doctor in 1829. His inaugural thesis, "On some Properties of the Impressions produced by Light on the Organ of Vision," raised him to a high rank among men of science. In 1834 he presented to the Royal Academy of Belgium his memorable work entitled 'Essay on a General Theory embracing all the Visual Appearances which succeed the Contemplation of Coloured Objects &c.' In 1835 Plateau was nominated Professor of Physics at the University of Ghent. As a teacher and as an investigator his work was characterized by the ingenuity with which he conceived and arranged his experiments, multiplied and varied them to an infinite extent, employing the most simple and ordinary means, such as are within the reach of every one.

In 1829, at the very outset of his scientific career, wishing to observe the consecutive effects of a strong excitation of the retina, Plateau, carried away by his passion for science, committed the fatal imprudence of fixing his eyes for nearly twenty seconds on the solar disk in its full brightness. This experiment gives us an idea of the energy and devotion with which Plateau pursued his studies. Unhappily it entailed upon him the most disastrous consequences: he remained blind for several days; his sight returned, but his eyes had received permanent injury, and his sight gradually became weaker, until, in 1843, fourteen years after his fatal experiment, it was entirely extinguished. This loss, irreparable as it was, could not check the scientific ardour of Plateau.

He was, before everything, an experimenter, and his principal instrument of research having failed him, he was determined to see through the eyes of others. As his experiments extended into several distinct domains of science, it was necessary for him to borrow the eyes of a great many different persons.

Plateau had been the pioneer in a special domain of physiological

optics; blind, he created a branch of experimental physics. In 1842 appeared, in the 'Bulletin' of the Royal Academy of Belgium, his work "On the Phenomena presented by a Mass of Free Liquid removed from the Action of Gravity." The experiments in question, characterized by beauty and ingenuity, were realized, as were most of his experiments, with the simplest possible appliances. This work was the starting-point of a long series of researches, not less remarkable, on Molecular Forces, published between 1849 and 1868 under the title "Experimental and Theoretrical Researches on the Figures of Equilibrium of a Mass of Liquid without Weight." In 1873 these papers were collected and published in two volumes, with the title 'Statique expérimentale et theorique des liquides soumis aux seules forces moléculaires.'

An activity so considerable in other fields of science did not, however, prevent him from pursuing the subject of his early labours and his first triumphs. His blindness naturally arrested his researches on physiological optics. The arrest, however, was but momentary: having drilled his helpers to this kind of investigation, he continued his studies, defended by the aid of new experiments the ideas which he had previously propounded, and, blind as he was, arrived at new and important results on luminous and coloured sensations. In 1872 he published the work "On the Measure of Physical Sensations," in which he controverted the views of E. H. Weber and Fechner. In 1878 he communicated to the Royal Academy of Science of Belgium a paper, "On a Law of Persistence of Impressions in the Eye," a work too little known, in which he completes and rectifies one of the propositions formulated fifty years before. From 1877 to the time of his death, which occurred on September 15, 1883, Plateau was engaged in compiling a valuable catalogue of all the papers he could meet with which bore on his special optical inquiries.

Plateau was a Commander of the Order of Leopold, Member of the Royal Academy of Belgium, Correspondent of the Institute of France, Member of the Academy of Sciences of Berlin, the Royal Academy of Sciences of Amsterdam, and the Royal Society of London. He was elected an Hon. Member of the Physical Society on February 8, 1879.

Charles Watkins Merrifield was born at Brighton in 1828. He received an appointment in the Education Department in 1847, and was soon promoted to the office of Examiner. In 1867 he was appointed Principal of the Royal School of Naval Architecture and Marine Engineering at South Kensington; but on the transfer of

this department to Greenwich he resumed his office as Examiner in the Education Department. He was for many years Honorary Secretary of the Royal Institute of Naval Architecture. He was elected a Fellow of the Royal Society in 1863. He was President of the Section of Mechanical Science at the British-Association Meeting in 1869. He had also held the office of President of the London Mathematical Society, and was one of the original Members of the Physical Society. His contributions to Mathematics were very numerous, and he wrote some important papers on sea-waves. For some years he frequently sat as Scientific Assessor in the Wreck-Court; he also served on several Royal Commissions. He died on the 1st of January last.

The Treasurer made the Financial Statement given below.

The Society then proceeded to the election of Officers and other Members of Council for the ensuing year. The Council was constituted as follows:—

President.—Prof. F. GUTHRIE, Ph.D., F.R.S.

Permanent Vice-Presidents.—Prof. W. G. ADAMS, M.A., F.R.S.; Prof. R. B. CLIFTON, M.A., F.R.S.; Prof. G. C. FOSTER, F.R.S.; Dr. J. H. GLADSTONE, F.R.S.; Sir WILLIAM THOMSON, LL.D., F.R.S.

Other Vice-Presidents.—Prof. W.E. AYRTON, F.R.S.; J. HOPKINSON, M.A., D.Sc., F.R.S.; Lord RAYLEIGH, M.A., F.R.S.; Prof. W. CHANDLER ROBERTS, F.R.S.

Secretaries.—Prof. A. W. REINOLD, M.A., F.R.S.; WALTER BAILY, M.A.

Treasurer.—Dr. E. ATKINSON.

Demonstrator.—Prof. F. GUTHRIE, Ph.D., F.R.S.

Other Members of Council.—SHELFORD BIDWELL, M.A., LL.B.; CONRAD W. COOKE; Prof. F. FULLER, M.A.; R. T. GLAZEBROOK, M.A., F.R.S.; R. J. LECKY, F.R.A.S.; Prof. O. J. LODGE, D.Sc.; Prof. H. MCLEOD, F.R.S.; HUGO MÜLLER, Ph.D., F.R.S.; Prof. J. PERRY, M.E.; Prof. S. P. THOMPSON, D.Sc.

The following was elected an Honorary Member:—

Prof. H. A. ROWLAND.

Votes of thanks were passed to the Lords of the Committee of Council on Education; to the PRESIDENT; and other OFFICERS of the Society; and to the AUDITORS.

THE TREASURER IN ACCOUNT WITH THE PHYSICAL SOCIETY, FROM JANUARY 1ST, 1882, TO DECEMBER 31ST, 1883.

Dr.	£ s. d.	£ s. d.
Balance in Bank		138 0 9
" hands of Treasurer		51 10 8
7 Entrance-Fees	7 0 0	
Subscriptions for 1880	3 0 0	
" 1881	7 0 0	
" 1882	18 0 0	
" 1883	115 0 0	
" 1884	3 0 0	
Life Composition	40 0 0	193 0 0
One year's Dividend on £400 Furness 4 per cent. Debenture Stock, less Income Tax	15 11 4	
One year's Dividend on £460 5 per cent. Midland Preference Stock, less Income Tax	22 7 6	
One year's Dividend on £200 3½ per cent. Metropolitan Board of Works Stock, less Income Tax	6 16 10	
One year's Dividend on £200 Lancaster Corporation Stock	7 16 2	52 11 10
Sales, less Commission		8 5 9
		£443 9 0

Cr.	£ s. d.	£ s. d.
Taylor and Francis:—		
Reprint of Joule's Scientific Papers	250 0 0	
Proceedings, vol. v. (parts 3 & 4)	62 1 6	
Postage and addressing	6 8 0	
Members' separate copies	13 0 6	
Miscellaneous printing	13 0 11	344 10 11
Reports of Meetings		10 0 0
Williams and Norgate:—Periodicals		9 8 0
Chapman:—Attendance and Petty Cash		4 6 11
Stationery and Printing		3 6 0
Petty Cash:—		
Mr. Reinold	1 14 2	
Mr. Baily	0 15 0	
Mr. Atkinson	1 14 0	4 3 2
Balance in Bank		62 8 0
" hands of Treasurer		5 6 0
		£443 9 0

London, February 1st, 1884.

Audited and found correct,

E. W. JONES,
F. W. BAYLY, } *Auditors.*

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY.

Assets.	£	s.	d.	Liabilities.	£	s.	d
Balance in Bank	62	8	0	Subscriptions in advance, for 1884	3	0	0
„ hands of Treasurer	5	6	0				
Estimated Subscriptions due	35	0	0				
£400 Furness 4 per cent. Debenture Stock	412	0	0	Balance	1510	14	
£460 Midland 5 per cent. Preference Stock	565	0	0				
£200 Metropolitan Stock	210	0	0				
£214 Lancaster Corporation Stock	214	0	0				
	£1513	14	0		£1513	14	0

We have examined the above Account, and also the Securities at the Bank, and find the same to be correct.

London, February 1st, 1884.

E. W. JONES,
F. W. BAYLY, } *Auditors.*

The following works have been presented to the Society :—

Carpenter, W. L. Energy in Nature.	The Author.
Du Bois-Raymond. Thierische Electricität. 2 vols.	Lady Siemens.
Dühring. Critische Geschichte der Mechanik.	"
Eisenlohr. Lehrbuch der Physik.	"
Gregory, G. Economy of Nature. 3 vols.	"
Kirchhoff. Gesammelte Abhandlung.	"
Loan Collection of Scientific Apparatus, 1876. Catalogue.	Dr. Guthrie.
Marbach. Physikalisches Lexikon. 6 vols.	Lady Siemens.
Mascart and Joubert (Atkinson). Electricity and Magnetism.	Dr. Atkinson.
Müller. Fortschritte der Physik.	Lady Siemens.
Regnault. Cours de Chemie. 4 vols.	"
Reuliaux. Der Constructeur.	"
Saunier. Treatise on Horology. 2 vols.	E. Rigg, Esq.
——. Watchmaker's Handbook.	"
Stanley. On Fluids.	The Author.
Technological Dictionary. Eng., Fr., & Ger. 3 vols.	Lady Siemens.
University College, London. Catalogue of Library. 3 vols.	The College.

The following are the principal Periodicals which have been presented to the Society :—

Great Britain and Ireland.

Royal Society. Proceedings, 29 vols.	Dr. Guthrie.
—— ——. Phil. Trans. 27 vols.	"
—— ——. Phil. Trans. Abridged by Hutton. 18 vols.	"
—— ——. Catalogue of Scientific Papers. 8 vols.	"
Royal Dublin Society. Transactions.	The Society.
Royal Institution. Proceedings. 5 vols.	Lady Siemens.
—— ——. Proceedings. 4 vols.	The Society.
Chemical Society. Journal. 20 vols.	Lady Siemens.
—— ——. ——. 13 vols.	Dr. Guthrie.
Physical Society. Proceedings. 2 vols.	Lady Siemens.
Society of Arts. Journal.	The Society.
Institute of Mechanical Engineers. Proceedings.	The Institute.
Society of Telegraph Eng. and Electricians. Journal.	The Society.
Cambridge Phil. Society. Proceedings. 4 vols.	The Society.
Society of Philosophical Experiments and Conversations. Minutes.	L. Clarke, Esq.
Philosophical Magazine. 44 vols.	Lady Siemens.
The Inventors' Record. 7 vols.	"
Stonyhurst College. Observations.	Prof. Perry.

Scientific Roll.	The Editor.
Kew Committee. Report.	The Committee.
Radcliffe Library, Oxford. Catalogue of Additions.	The Library.
University College, London. Calendar.	The College.
Glasgow University Calendar.	The University.

America.

The Smithsonian Institution. Report.	The Institution.
Johns Hopkins University Circulars. 12 Nos.	The University.
American Philosophical Society. Proceedings, 5 vols.	Lady Siemens.

France.

Société Française de Physique. Ordres du jour.	The Society.
——— ——— ———. Séances.	"
Bureau des Longitudes. Annuaire.	The Bureau.
Les Mondes. 23 vols.	Lady Siemens.

Germany.

Repertorium für Experimental Physik. 21 vols.	Lady Siemens.
Jahresberichte Fortschritte der Chemie. 3 vols.	"
——— ——— der Physik. 30 vols.	"
Centralblatt Chemisches. 31 vols.	Dr. Guthrie.
Gmelin. Handbook of Chemistry. 8 vols.	Lady Siemens.

Japan.

Science Department, Tokio. Memoirs.	The Department.
Seismological Society of Japan. Proceedings.	The Society.

Norway.

Norwegian North-Atlantic Expedition. 3 vols.	The Committee.

Russia.

St. Petersburg Journal of Chemistry and Physics.	The Editors.